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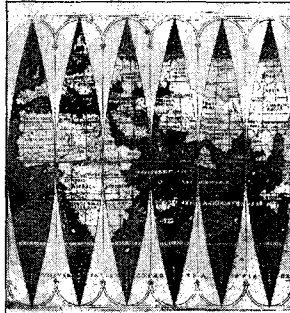
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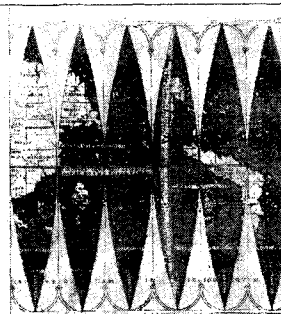
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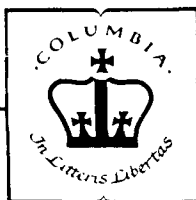
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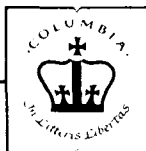
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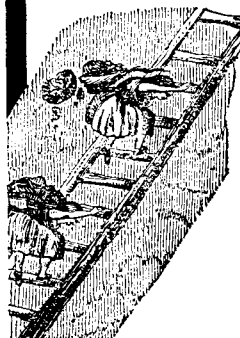
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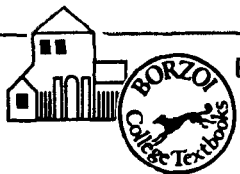
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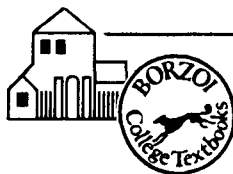
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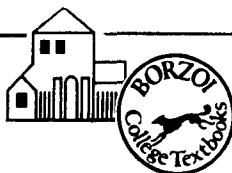
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The Future of the Past

C. VANN WOODWARD

Time present and time past
Are both perhaps present in time future,
And time future contained in time past.
T. S. Eliot, *Burnt Norton*

THE fortunes and vitalities of the learned disciplines, history as well as the others, vary considerably from period to period. Decades of confidence and fulfillment are followed by eras of hesitation and marking time. The relative status and prestige of the disciplines change accordingly. Without attempting to explain the phenomenon, I would contend that the profession of history in America has enjoyed a period of exceptional felicity, in fact, something of a boom period, during the two decades following the Second World War.

This estimate is not based on an assessment of the quality and distinction of the scholarship of that period, though they are surely not inconsiderable. It is based, rather, on evidence indicating a sense of corporate well-being, justified or not, that pervaded the guild in those years. Among the components of felicity were some that were widely shared in the academic world: the rising status of college professors in general, for example, and a revived prestige of the humanities. But apart from these shared advantages were others peculiar to the professional historian. They included emancipation from some old feelings of inferiority. One sense of inferiority derived from doubts about the validity of historical knowledge,

► Mr. Woodward, Sterling Professor of History, Yale University, gave this presidential address at the annual meeting of the American Historical Association, Washington, D. C., December 29, 1969.

another from relations between history and other disciplines, and a third from relations between the historian and his public, both academic and nonacademic. Insecurities in each of these areas had haunted historians for years. But after the Second World War, a rough consensus on the validity of historical knowledge restored confidence in the integrity of the craft. Relations with sister disciplines, as well as status and prestige among them, improved and strengthened the position of history in American academic life. Beyond the academic walls professional historians began for the first time to capture a reading public to rival that of the amateurs. The substance of things hoped for materialized in the form of larger enrollments, higher salaries, and more substantial royalty checks. But before these tangible rewards, and more important, came the apparent solution of a series of intellectual problems within the guild.¹

"Solution" is perhaps too strong a word. American historians in the postwar years were more disposed to shrug off than to solve abstruse problems of theory. Unaccustomed to theoretical argument and lagging a generation behind European thought on such problems, they were impatient with these subtleties. More than anything else they wanted to break out of the defensive and subservient position in which they had been cornered. The relativists of the profession in the prewar years had disputed the historians' claim to objectivity. Social scientists, with whom historians had eagerly sought alliance in earlier years, had become patronizing and sometimes contemptuous toward historical scholarship. Historical relativism combined with social science to imprison the historian in the contemporary world and to shift his allegiance and his attention from the past to the present. A powerful school of progressive historians demanded that history be written in accordance with some vision of the future. Critics not only called the integrity of the past in question and subordinated the past to concern for the present and the future, but they impugned the validity of historical knowledge itself and relegated the profession devoted to its study to an inferior status among the disciplines. It was no wonder that a leading historian could complain in 1947 of "the confusions in which most historical students have been tossing."² The morale of the guild and the self-esteem and confidence of its members were in disarray, and some despaired of the beleaguered and defenseless plight of the craft.

A spirited and somewhat combative reaction soon emerged among historians. Putting aside niceties of consistency, they began to assert that if physicists could live with relativity, historians could live with relativism. If relativism was unavoidable, they would opt for something they imperiously called "objective relativism." Granting the impossibility of certainty, they maintained that history was "a quest for wisdom instead of a quest for certainty."³ If the alliance with social

¹ The best history of the profession in America during this period is John Higham *et al.*, *History: The Development of Historical Studies in the United States* (Englewood Cliffs, N. J., 1965).

² Frederic C. Lane, in *Journal of Economic History*, VII (May 1947), 83.

³ Social Science Research Council, *The Social Sciences in Historical Study* (New York, 1954), 1-17.

sciences were to continue, it must be on terms of equality and not subservience. Roy F. Nichols caught the new mood in "a declaration of intellectual independence" published in the fall of 1948. "History is not art, science, or literature," he insisted,

it is *sui generis*. It is a division of knowledge with its own character and methods. . . . It is time for historians to be more positive about their functions, their objectives, and their methods. It is time to stop living by other people's wits, by frantically seeking to adopt other people's jargon, by humbly seeking to be recognized as faithful and reasonably satisfactory handmaids worthy of Thursday afternoons and alternate Sundays on which to do what they really wish.

Nichols wanted to release history from both "the clutches of heedless optimism" and "the slavery of present-mindedness," the myth of the future as well as imprisonment in the present.⁴

The call for autonomy struck a responsive chord among historians. Just as the founding fathers of the scientific school had felt it necessary to assert their independence from literature and philosophy, and their successors from nineteenth-century science, so a declaration of independence from the social sciences was now in order. Thus, as John Higham observes, "the outlook of the professional historian had come full circle," and in the process "reconstituted the historian's autonomous identity."⁵ Autonomy did not preclude alliances on more equal terms, however, and those with the social sciences became more cordial. There were many who wished to keep a foot in both camps. H. Stuart Hughes, for one, took "pride in the mediating character" of history, its "half-scientific, half-artistic nature," and thought that "the historian's supreme technical virtuosity lies in fusing the new method of social and psychological analysis with his traditional storytelling function."⁶ Historians thus claimed the best of both worlds. They revived romantic notions such as intuition, insight, empathy, and imagination and proposed at the same time to use them as freely as they did scientific concepts of analysis. They staked out neutral territory and posed not only as mediators between past and present but as conciliators between the "two cultures" and friendly patrons of the third, the social sciences. It was, all things considered, rather high ground that they took and withal a rather privileged position that they claimed.

For a time the historian was fortunate in attracting philosophers and logicians (to whose dialogues about history he was normally deaf) willing to support the claim of historical autonomy and to defend the craft from the philosophical attack on historical explanation.⁷ It was also gratifying to find theologians (an-

⁴ Roy F. Nichols, "Postwar Reorientation of Historical Thinking," *American Historical Review*, LIV (Oct. 1948), 78-89.

⁵ Higham *et al.*, *History*, 135.

⁶ H. Stuart Hughes, *History as Art and as Science: Twin Vistas on the Past* (New York, 1964), 3, 77; see also Henry Steele Commager, *The Nature and Study of History* (Columbus, Ohio, 1965).

⁷ William H. Dray, *Laus and Explanation in History* (London, 1957), and *Philosophical Analysis and History*, ed. *id.* (New York, 1961), esp. the essays by Sir Isaiah Berlin, W. H. Walsh, J. A. Passmore, Alan Donagan, and Louis O. Mink.

other group to whom historians were usually deaf) of the stature of Reinhold Niebuhr who took historians seriously and employed their findings respectfully.⁸ There were even social scientists who complained of the "a-historical character" of behavioral studies and conceived of lessons to be learned from the historian.⁹ Historians assured each other that Western culture was "the historical culture par excellence," that the present was "the most historically minded of all ages," that historical consciousness was its distinguishing characteristic. They quoted Friedrich Meinecke as calling this "the greatest spiritual revolution which Western thought has undergone" in modern times, and Johan Huizinga as saying that "historical thinking has entered our very blood."¹⁰ By 1965 Higham could note in his admirable history of the profession a shedding of old inferiority feelings, a quickening of vitality, a revival of confidence, and a "restoration of intellectual self-respect that has taken place since 1945."¹¹

In addition to the new confidence in their calling, American historians whose subject was American history discovered among themselves as well as their public a gratifying shift in attitude toward their field of study. In his presidential address to the American Historical Association in 1950, Samuel Eliot Morison noted with satisfaction "a decided change of attitude toward our past, a friendly, almost affectionate attitude, as contrasted with the cynical, almost hateful one of young intellectuals" in earlier years.¹² The new friendliness and affection toward the American past found confirmation in other presidential addresses and in autobiographies by historians prominent in the period. "For the student of United States history," wrote the late Arthur M. Schlesinger, "there is in addition the special joy of discovery and understanding how one's own people have reached their present condition; why . . . we have come to behave like Americans," and how we have "held up a lamp to Europe and, more recently, to Asia and Africa."¹³ Recent autobiographies of other American historians similarly reflect nostalgic affection for the American past, reconciliation with the present, and optimism about the future.¹⁴

A "special joy" in discovering how Americans reached their present condition and came to behave like Americans would seem to imply a special satisfaction in the condition of Americans and the way Americans behave. And a friendly and

⁸ Reinhold Niebuhr, *The Irony of American History* (New York, 1952).

⁹ Robert A. Dahl, "The Behavioral Approach," *American Political Science Review*, LV (Dec. 1961), 771.

¹⁰ E. H. Carr, *What Is History?* (London, 1961), 129; John Luckacs, *Historical Consciousness, or the Remembered Past* (New York, 1968), 18, 23.

¹¹ Higham *et al.*, *History*, 132-37.

¹² Samuel Eliot Morison, "Faith of a Historian," *American Historical Review*, LVI (Jan. 1951), 272; see also Richard Hofstadter, *The Progressive Historians: Turner, Beard, and Parrington* (New York, 1968), 92.

¹³ Arthur M. Schlesinger, *In Retrospect: The History of a Historian* (New York, 1963), 203.

¹⁴ John D. Hicks, *My Life with History* (Lincoln, Neb., 1968), 354; Roy F. Nichols, *A Historian's Progress* (New York, 1968), 299-300; Dexter Perkins, *Yield of the Years* (Boston, 1969), 204, 233, 235.

affectionate identification of the present with the past suggests a genial compatibility between the two that bridges the gulf between generations, implies a vitality and relevance of traditional ways, and assumes a fundamentally benign continuity of institutions and ideas. It was essentially a conservative view of the past. The school of interpretation that renounced and succeeded the progressive school usually, though not invariably, found the friendly attitude toward the past congenial to its views.¹⁵ These historians stressed consensus rather than conflict and emphasized stability and homogeneity rather than change and contrast. They dwelt rather nostalgically upon what was appealing or virtuous in the American past, and rarely on the darker, more violent, and tragic aspects of the national experience.

Whether the new attitude toward the American past or the rejuvenated confidence of the American history profession explains it or not, the fact is that history has enjoyed a boom period that coincides with these two developments in the profession. History stock was rising in American culture at large. The boom was most pronounced in the academic market place, but it was not confined to that world. It also penetrated the world of the publishers, and to some degree the worlds of the foundations, the intellectuals, even the politicians, not to mention a large and unsophisticated lay reading public that likes its history with colored illustrations. This picture admittedly runs counter to that painted in 1964 by a distinguished committee of the American Historical Association. Concerned particularly over what they felt to be neglect by the foundations, this committee complained that history was "too little honored," "too much taken for granted," and "consistently underestimated." But even they noted with satisfaction that "history and English literature are the two most powerful major studies in many of our leading universities and colleges—and the prime elective studies too."¹⁶

Perhaps it would be well first to clear up some confusion about that not wholly captive nor altogether predictable market, the college student, graduate and undergraduate. During the huge expansion in higher education that took place in the first half of this century, the numbers of students in both the humanities and the sciences increased enormously. But while the humanities as a whole and the natural sciences suffered a relative decline in the percentage of degrees granted, history consistently held its own in this respect.¹⁷ English and foreign languages declined to about a quarter of their former relative strength; the natural sciences suffered smaller losses, while the social sciences scored rapid gains. In the meantime, from 1901 to 1953, history succeeded in maintaining a constant level. In the next sixteen years, down through 1968, all of these fields

¹⁵ Richard Hofstadter is certainly one exception.

¹⁶ American Council of Learned Societies, *Report of the Commission on the Humanities* (New York, 1964), 114.

¹⁷ W. David Maxwell, "A Methodological Hypothesis for the Plight of the Humanities," *American Association of University Professors Bulletin*, LIV (Spring 1968), 78-80.

have enjoyed a relative increase in the percentage of degrees granted, but none so large as history and the language group, both of which nearly doubled the percentage they granted in 1953¹⁸ So much for numbers and percentages; quality is a more difficult matter. We are informed on good authority, however, that in the competitive recruitment of talent, history has been able to win to graduate study a larger percentage of Seniors who distinguished themselves in that field than it once did.¹⁹

The more students, the more professors. The membership of the American Historical Association, stalled for some years before 1953 at about five thousand, more than tripled in the next dozen years. The prodigious increase in membership was matched by a proliferation of activities, staff members, committees, and publications. The growing size and the additional number of the Association's *Review* reflected in part the enormous increase in the number of history books written and the obligation to review them. The total number of history titles published in the United States in 1968 was three times the total for 1950.²⁰ The number of periodicals in the country devoted to history of all kinds, including popular, patriotic, and local, has almost doubled since 1946.²¹ The responsiveness of the foundations to the appeal of historians, though still quite niggardly, multiplied sevenfold in five years.²² In the 1960's two opposing candidates for President of the United States wrote, or at least published, history books of a sort with, in one case the presumed and in the other the explicitly avowed, intent of enhancing the appeal of the politician-authors as intellectuals.²³ And in two administrations of that decade, the White House "intellectual," a new adornment of the executive office, was a prominent American historian. Such were the emoluments, the bounties, and the bonuses of the great history boom.

Booms do not last forever, and it would now seem prudent to examine the prospects and portents of the recent one. In so doing I would try to avoid the role of alarmist. I am not charging the profession at large with giddiness or com-

¹⁸ Paul L. Ward, "The Plight of the Humanities," *ibid.* (Autumn 1968), 397.

¹⁹ Dexter Perkins and John L. Snell, *The Education of Historians in the United States* (New York, 1962), 38.

²⁰ *American Library Annual* (New York, 1950), 80; *Bowker Annual Library and Book Information* (New York, 1969), 36. Granted much imprecision about what constituted a "history" title, the number so designated increased from 516 in 1950 to 1,528 in 1968.

²¹ *Ulrich's International Periodicals: A Classified Guide to a Selected List of Current Periodicals, Foreign and Domestic*, ed. Eileen Graves (New York, 1947-69). Sixty-six new journals were founded between 1947 and 1956.

²² "Foundation Grants," *Foundation News* (Mar., Sept. 1963; Mar., Sept. 1968). This includes only grants over ten thousand dollars and does not include renewals or grants not reported to the *News*. It does include grants for restoration and preservation of historic sites. Grants increased from one million dollars in 1963 to seven million dollars in 1968, but they fell precipitantly to three million dollars in 1969. (*Ibid.* [Sept.-Oct. 1969].)

²³ John F. Kennedy, *Profiles in Courage* (New York, 1956); Richard M. Nixon, *Six Crises* (New York, 1962). In the preface of the latter work (p. xi), the author writes that his opponent had advised him that such a book "tends to elevate [the politician] in popular esteem to the respected status of an 'intellectual.'"

placency or delusions of grandeur—failings often characteristic of boom psychology. It is reassuring to remember that throughout this period of boom and bustle, inflated markets and popular attention, there have been some American historians who have gone about their self-assigned tasks of scholarship undeterred, drawing mainly on inner resources. The quality and importance of the work they have done will probably bear favorable comparison with the historical scholarship of any previous period. Granting all that, I do feel obliged, nevertheless, to report certain signs of trouble. They are confined mainly to three overlapping areas: the academic world, the intellectual community, and the profession itself.

Within the academy the conventional barometer of departmental fortunes (for what it is worth) is student patronage, and in significant quarters that barometer has been falling lately for history. Measured by percentage of degrees granted, as we have seen, history has for the last sixteen years enjoyed an unprecedented increase in students. This may well continue for some time in terms of national averages. But a sampling of indicators that lie back of the degree stage, a look at figures on course enrollments, departmental majors, and the attitudes of precollege students would suggest a somewhat different prospect. A survey of the sixty-four American institutions that currently graduate the highest percentage of their students in arts and sciences as history majors (those with at least three times the national average) indicates that well over half of those responding, twenty-five out of forty-three, suffered a decline in student patronage in the last two years. In the minority group of eighteen that reported no significant change or actual increase of student interest fall most of the small denominational colleges in the oddly assorted sample thus defined. On the other hand heavy losses in undergraduate history course enrollment in the last two academic years, losses ranging from 27 to 34 per cent, occurred at Harvard, Yale, Stanford, and Amherst. Among eastern institutions, Princeton, Brandeis, Smith, Swarthmore, Williams, Hamilton, Haverford, Georgetown, and Fordham reported substantial but smaller losses in their history courses. In other parts of the country, with a much smaller proportion of colleges graduating the specified percentage of history majors, several institutions of high quality included in the survey also reported declining student interest in history. Generalizations based on these findings are difficult to formulate and can be quite misleading. Several institutions cite local and temporary conditions to explain their losses. These changes, moreover, are too recent to provide any reliable projection for the future. They do occur, however, often in their most pronounced form, in some of the most sensitive zones of American academic life and can scarcely be dismissed as insignificant.²⁴

More indicative about the future of the past and future student interest in

²⁴ The decline in student patronage of history at Oxford and Cambridge Universities has also been sharp since the Second World War, but it was spread over a longer period. The comparisons in the American survey are between figures for 1966-1967 and 1968-1969.

history are the attitudes of the oncoming generation of college students. A recent poll shows that of the twenty-one subjects in their curriculum on which they were questioned, American high school students regarded history as the "most irrelevant."²⁵ The planners of their curriculums and the writers of their textbooks would seem to share the aversion of the students. Time and attention once devoted to history have rapidly given way to current affairs and social studies. Serving as a member of the California Statewide Social Sciences Study Committee, Charles Sellers reports that, "Virtually no one except the history professors on this large and representative committee saw much value in retaining history in the curriculum at all."²⁶

The changing whims and cultural styles of college students and adolescents have recently become the subject of a massive literature of which I am not a master. The aversion to history may be a minor symptom of more arcane ailments. The monotonous recurrence of the word "relevance" in student parlance does, however, require some notice from the historian. Highly subjective and capable of as many meanings as it has users, the term "relevance" seems to express an inarticulate existentialism, a headlong immediatism of the "here" and "now" that is impatient with any "there" and "then" that do not have an obvious bearing on present preoccupations or personal problems. In accounting for the defection from history courses, Harvard mentions "a very substantial increase" of undergraduate concentrators in the government department; Yale and several others refer to a rising popularity of psychology, Princeton to a boom in sociology. These shifts of undergraduate tastes may, of course, be quite temporary. The current vogue of combining Cavalier hairstyles with Roundhead earnestness may well revert once more to tonsorial roundheadedness and attitudinal cavaliness. As a cultural indicator, the undergraduate passion for "relevance" may have less to do with the long-range future of the past than with the short-range future of the job market for historians.

The declining status of history in the intellectual community at large is a more serious matter. Most disturbing of all is the antihistory animus that emanates from quarters generally assumed to be in friendly alliance with history: the arts and humanities. The tradition of literary animus may be traced back to Friedrich Nietzsche, Henrik Ibsen, and André Gide, but nothing before quite equals the rebellion against history among more recent and contemporary men of letters. Foremost among the rebels are the French existentialists André Malraux, Albert Camus, and the early Jean-Paul Sartre, though their attitudes have precedents in Paul Valéry, James Joyce, and Thomas Mann. For Joyce's Stephen Dedalus history was a "nightmare" from which he must awaken. The tyranny of historical

²⁵ Poll by Louis Harris and Associates, Inc., of "100 schools in representative cities, suburbs, small towns and rural areas," for *Life*, LVI (May 16, 1969), 23, 31.

²⁶ Charles G. Sellers, "Is History on the Way out of the Schools and Do Historians Care?" *Social Education*, XXXIII (May 1969), 510.

consciousness is a recurrent theme in modern literature. It is implicit in William Carlos Williams' plea for immediacy: "History, history! We fools, what do we know or care?" And it is explicit in his renunciation of history as "a tyranny over the souls of the dead—and so the imaginations of the living." Camus, a cultural hero of the young, spoke candidly of his indifference to history and contemptuously of "the evil of history." Sartre wove the same bias through his early fiction and philosophy.²⁷ What some of the foremost men of letters in our culture have been saying for some time without our special heeding is that history as currently written is bland, banal, or Philistine, that it is often morally obtuse, aesthetically archaic, and intellectually insipid. The historian's pretensions as artist are regarded as pathetic if not ludicrous. Among fashionable playwrights and fiction writers such as Kingsley Amis, Angus Wilson, and Edward Albee these attitudes have become a cliché. In their plays and novels the historian is a stock figure of ridicule, regularly kicked about the stage as part of the evening's entertainment.²⁸

The scientists have been more circumspect and less vehement and articulate in verbalizing their aversions and suspicions toward history. But as the anthropologists move in from the far side and the psychologists, sociologists, and political scientists encroach from the hither side upon the territory of human experience that historians normally think of as their private preserve, cries of impatience and dismay arise from the invaders on both flanks. Complaints of methodological naïveté, conceptual vagueness, ambiguous premises, and outmoded generalizations pour in. If this is the science to which historians profess part-time allegiance, they are saying, it is bad science or outmoded and archaic science—late nineteenth-century science at best. The space devoted to history in the *International Encyclopedia of the Social Sciences*, published in 1968, is strikingly less than the amount in its precursor of 1930. Biographies of historians are markedly fewer than those in other disciplines represented, and of the fifty historians included among the six hundred subjects of biographies only six did their work in the United States.²⁹ What scientists are implying in various ways is that history as now practiced is as far out of touch with modern science as men of letters are explicitly saying that history is isolated or alienated from modern art.³⁰

Among philosophers there are still reputable champions such as Sir Isaiah Berlin, who put up a formidable defense for the autonomy of history as a disci-

²⁷ Sartre underwent a later "conversion to history" according to Leonard Krieger, "History and Existentialism in Sartre," in *The Critical Spirit: Essays in Honor of Herbert Marcuse*, ed. Kurt H. Wolff and Barrington Moore, Jr. (Boston, 1967), 239–66. Camus himself came eventually to base values in history.

²⁸ Hayden V. White, "The Burden of History," *History and Theory*, V (No. 2, 1966), 115–23.

²⁹ See review of the *International Encyclopedia of the Social Sciences*, ed. David L. Sills (17 vols., New York, 1968), by Thomas C. Cochran in *American Historical Review*, LXXIV (June 1969), 1573–76.

³⁰ The critique of history by behavioralists is ably presented by Robert F. Berkhofer, Jr., *A Behavioral Approach to Historical Analysis* (New York, 1969).

pline *sui generis*.³¹ Others are increasingly skeptical of these claims. They are suggesting that the middle ground the historian occupied in the nineteenth century between the romantic artists' fear of science and the positivistic scientists' ignorance of art may no longer exist, nor may the extreme dissimilarity of art and science still be assumed by the historian, nor may his self-appointed function as mediator between the two. It has even been suggested that the historian's prestige among intellectuals of the nineteenth century was culturally determined, that the conception of history then current was itself the product of ephemeral historical circumstances, the passing of which may well have deprived history of its status, its autonomy, and its traditional defenses. "In short," according to one harsh judgment, "everywhere there is resentment over what appears to be the historian's bad faith in claiming the privileges of both the artist and the scientist while refusing to submit to critical standards currently obtaining in either art or science."³²

Some of these attacks, including the last quoted, were exaggerated and open to effective rejoinder, and some of the critics and their disciplines were not above criticism themselves. But in so far as contemporary historians have been at all aware of criticisms by artists, scientists, and philosophers, they have often (though not always) fallen back on the old Fabian strategy of fending off the scientists with the assertion that history was a kind of art given to the uses of intuition beyond the ken of science, and fending off the artists by maintaining that history was a kind of science limited by analytical methods inappropriate to artistic manipulation. A "guild," a "craft," we quaintly call it. Half scientist, half artist, the historian was a sort of centaur among the disciplines, a centaur with wings some would have it, the only academic discipline endowed with an authentic muse. Incapable of sustaining such fanciful poses, or unaware of the attacks that provoked them, the great majority of American historians became increasingly preoccupied with their fields of specialization and intradisciplinary matters. With some exceptions, by the mid-twentieth century American historians carried little weight and took little part in discussions preoccupying the intellectual community at large, even discussions pertaining to their own discipline. Whatever contemporary paintings they hung on their walls or modern literature they kept on their shelves, little of the spirit that informs these arts seemed to enter into the monographs the historians wrote.

More and more, historians in America sought refuge in their supposedly captive audience of students, or increasingly in the growing public they enjoyed among the laity. In the latter, many guildsmen took a special pride. Unlike other disciplines, they pointed out, history abjured a specialized jargon and spoke the language of the people. While other arts and sciences were growing more abstruse

³¹ Sir Isaiah Berlin, "History and Theory: The Concept of Scientific Theory," *History and Theory*, I (No. 1, 1960), 1-31.

³² White, "Burden of History," 112.

and occult, history was becoming more popular.³³ Excited by the prospect, historians took to the airwaves and the platform with campaigns of salesmanship. Within the guild a movement led by Allan Nevins and Conyers Read narrowly failed to secure sponsorship by the American Historical Association for their dream of a popular magazine of history. But the dream later materialized as a commercial success of fabulous proportions—slick, handsome, well edited, and bland.³⁴ Historians also reached the laity through the numerous book clubs, Civil War round tables, state and local history societies, and civic enthusiasm for restoring historic sites and commemorating centennials. The efforts to please popular taste and court popular esteem tended to encourage the qualities of blandness and banality complained of by the critics of history. They also tended to diminish the esteem in which the craft was held by sister disciplines and to put even more distance between historians and the intellectual community.

There is, moreover, a false security about the refuge sought in popular esteem. It is true that history as romantic entertainment or as “heritage” sustaining national pride has enjoyed great waves of popularity among Americans, especially those of the first half of the nineteenth century.³⁵ The assumption of a solid and continuing patronage for the profession anchored in a national devotion to history, however, fails to reckon with some old traits of national character. Accused by their critics of “not learning from history” and being indifferent to the past, spokesmen of the student movement reply: “But we say that there is no historical precedent for our generation.”³⁶ So far as historical precedent for their attitudes toward the past is concerned, American students are poorly informed. There are many precedents for their attitude. They go back at least to the Jeffersonians, for some of whom the past seemed both misleading and irrelevant. For Thomas Jefferson himself the present was as independent of the past as the United States from England, and had no more authority over it. “We may consider each generation as a distinct nation,” he wrote, and Jefferson once reckoned the length of a generation as nineteen years and the legitimacy of all laws and institutions, without consent of the living, as of the same duration.³⁷ Alexis de Tocqueville unerringly picked up the theme of generational disjuncture: “the tie that unites one generation to another is relaxed or broken”; he wrote, “every man there

³³ See the presidential address of Allan Nevins, “Not Capulets, Not Montagus,” *American Historical Review*, LXV (Jan. 1960), 253–70, esp. 255–57.

³⁴ Higham *et al.*, *History*, 80–84.

³⁵ George H. Callcott, *History in the United States, 1800–1860* (Baltimore, 1969), esp. Chap. 11.

³⁶ Gregory H. Wierzynski summarizes interviews with students on twenty campuses in *Fortune*, LXXIX (Jan. 1969), 146.

³⁷ Daniel Boorstin, *The Lost World of Thomas Jefferson* (New York, 1948), 169, 204, 205, and Chap. IV, Sec. 4, entitled “The Sovereignty of the Present Generation”; but cf. H. Trevor Colbourn, *The Lamp of Experience: Whig History and the Intellectual Origins of the American Revolution* (Chapel Hill, N. C., 1963), 158, 167. The quoted statements of Jefferson do not do justice to his respect for history. On eighteenth-century attitudes toward history, see Peter Gay, *The Enlightenment: An Interpretation* (2 vols. to date, New York, 1966, 1969), II, 368–96.

loses all trace of the ideas of his forefathers or takes no heed of them." Or again, "In America, society seems to live from hand to mouth, like an army in the field."³⁸ According to R. W. B. Lewis, "the American myth saw life and history as just beginning . . . a divinely granted second chance for the human race." He quotes the *Democratic Review* of 1839 as saying, "Our national birth was the beginning of a new history . . . which separates us from the past and connects us with the future only."³⁹

Our eighteenth-century ancestors, according to William Bartram, had a ceremony called the "busk," in which a whole town would periodically turn out to make a common bonfire of everything old, outworn, and discarded. Henry David Thoreau made a metaphoric extension of the busk into a social philosophy of "purifying destruction." "I have lived some thirty years on this planet," wrote Thoreau, "and I have yet to hear the first syllable of valuable or even earnest advice from my seniors," that is, anyone over thirty.⁴⁰ It may have been the tradition of the busk that inspired Nathaniel Hawthorne's fantasy of 1844, "Earth's Holocaust," a terrifying metaphor of the American urge toward purifying destruction. On a vast western prairie a huge throng gathered to light a cosmic bonfire of the world's "outworn trumpery," including the whole body of European literature and philosophy. "Now," declared their leader, "we shall get rid of the weight of dead men's thoughts."⁴¹ Even Ralph Waldo Emerson, for all his apostrophe to history, declared that "All inquiry into antiquity . . . is the desire to do away with this . . . preposterous There or Then, and introduce in its place the Here and the Now."⁴² The "Now Generation" is not without authentic antecedents. It would be possible to follow the theme of indifference or hostility toward the past through the corpus of American literature down to contemporary writers directly influenced by the existentialists. The fact is that if Americans may be said to have been born Lockceans, they can in this particular respect as readily be described as having been born existentialists—without the obligation to Sartre they owe to John Locke.⁴³

In addition to this heritage of attitudes from the past about the past, Americans are the creators of wholly new disruptions between past and present. Even the committee of historians who a few years ago deplored and deprecated the effect

³⁸ Alexis de Tocqueville, *Democracy in America*, ed. Phillips Bradley (2 vols., New York, 1956), II, 4; I, 212.

³⁹ R. W. B. Lewis, *The American Adam* (Chicago, 1955), 5, and Chap. 1, entitled "The Case against the Past."

⁴⁰ Quoted in Thornton Wilder, "Toward an American Language," *Atlantic*, CXC (July 1952), 29.

⁴¹ The difference between Hawthorne and Thoreau, as Lewis points out, was that the former knew that the holocaust did not touch the real source of oppression, the human heart. (Lewis, *American Adam*, 13-15.)

⁴² *The Complete Essays and Other Writings of Ralph Waldo Emerson*, ed. Brooks Atkinson (New York, 1940), 127.

⁴³ For one of several southern exceptions to the national norm, see William Faulkner's Gavin Stevens in *Intruder in the Dust*: "The past is never dead. It is not even past."

of these forces admitted that "the world is now moving so fast that history of the knowable past seems to Americans more than ever irrelevant, out of date and useless."⁴⁴ Unlike agrarian, or craft, or commercial societies, industrial society, in the opinion of J. H. Plumb, really needs no past. For the industrial society, he writes, "The past becomes . . . a matter of curiosity, of nostalgia, a sentimentality. . . . The strength of the past in all aspects of life is far, far weaker than it was a generation ago: indeed few societies have ever had a past in such galloping dissolution as this."⁴⁵ But Plumb is evidently thinking of contemporary Britain or Europe, and we are constantly told that America and Europe are no longer living in the same historical era, that the United States has already broken through into something called the "postindustrial" or the "technetronic" age and is in daily confrontation with the unprecedented. In the American experience, other peoples of the world can read, for better or for worse, what is in store for them in the future—for most of them a very remote future—in which Americans are already living.

This future already exhibits more disjunction from the past than did the industrial era. One English interpretation of the first lunar landing, doubtless moved to rashness by the spectacle, pronounced it "the most brutal break that the world has known with its past 400 million years."⁴⁶ For a Carnegie, a Rhodes, a Krupp, a Rockefeller, or even a Henry Ford the past was still fairly rich with sanctions. For their counterparts (if any) of the "postindustrial" world it is hard to see what the past provides in the way of sanctions. The new spiritual environment, on the other hand, is not wholly strange or entirely uncongenial to the American. "Americans are abstract," writes Thornton Wilder. "They are disconnected," from place as well as from past. "There is only one way in which an American can feel himself to be in relation to other Americans—when he is united with them in a project, caught up in an idea and propelled with them toward the future. . . . 'I am,' he says, 'because my plans characterize me.'"⁴⁷ An essential element of American identity, one of "the principal sources of identity strength," according to Erik Erikson, is "a sense of anticipated future."⁴⁸ And in the words of the *Democratic Review* already quoted, our history "separates us from the past and connects us with the future only." Henry James was under the impression that America "had no past" and as "the next best thing" was hell-bent on substituting "a magnificent compensatory future."⁴⁹ Perhaps the outworn faith in progress was only one expression of a deeper national commit-

⁴⁴ American Council of Learned Societies, *Report of the Commission on the Humanities*, 119.

⁴⁵ J. H. Plumb, *The Death of the Past* (Boston, 1970), 14-15; see also *ibid.*, 42-44.

⁴⁶ *Economist*, July 19, 1969, 13.

⁴⁷ From his Charles Eliot Norton Lectures at Harvard University, in *Atlantic*, CXC (July 1952), 31.

⁴⁸ Erik Erikson, "Memorandum on Youth," in *Toward the Year 2000: Work in Progress*, ed. Daniel Bell (Boston, 1968), 231-32.

⁴⁹ Quoted in H. G. Wells, *The Future of America* (New York, 1906), 5.

ment. What William James saw as our "platitudinous optimism" and H. G. Wells called our "optimistic fatalism" could become in Henry Adams a rather lurid and apocalyptic catastrophism without essential inconsistency with the future-oriented bent of American character.⁵⁰

A profession that sets itself up as custodian of the past among a people with such peculiar attitudes toward the past, a people that has characteristically sought identity in the future, would seem to have little ground for complacency—about either the future of the past or the future of the craft that deals with the past. Complacency in these matters would seem all the more inappropriate in a period of radical disjunctures between past and present, and so would withdrawal of the historian from the continuous discussions of these disjunctures carried on in the intellectual community. Deafness or indifference toward criticism of the guild, whether it comes from artists, scientists, or philosophers, or from our own students, would appear to be singularly perilous at this time. The last resort of Philistinism would be for professional historians to take refuge from intellectual problems and critical attacks in what remains of popular sentimentality and nostalgia about the past.

Plumb attempts to draw "a sharp distinction between the past and history." The past as he conceives of it is always used to sanction or sanctify authority, to control or motivate societies or classes, to endow elites and nations with a sense of destiny and mission, and therefore to bemuse and coerce and exploit. "Nothing has been so corruptly used as concepts of the past," he writes, and he is ready "to toll the bell for the past which is dying." The demise of the past, however, "does not deny a future for history." History is not an ideology, and it is not the past. It is an intellectual process, a discipline that is still growing. Its future and true function are "to cleanse the story of mankind from the deceiving visions of a purposeful past."⁵¹

There are admittedly some ambiguities in this contrast between history and the past. But American historians, particularly those whose subject is the history of their own country, have often been careless and sometimes even oblivious of the more elemental distinctions between the two. In their quest for a "usable past" they have fallen into what J. R. Pole calls "the American extension of the Whig interpretation of history."⁵² An instrumentalist view of historiography, this interpretation regards history as an instrument of political or social action. It assumes that the United States stands for certain values and that it is the duty of the historian in his study of the past to discover, record, and celebrate these

⁵⁰ On the historian and the future, see Henry Adams, *The Education of Henry Adams* (Boston, 1918), 395. Offering Tocqueville and Adams as examples, Hofstadter has observed that, "At their best, the interpretative historians have gone to the past with some passionate concern for the future. . . ." (*Progressive Historians*, 465.)

⁵¹ Plumb, *Death of the Past*, 11, 17, 50.

⁵² J. R. Pole, "The American Past: Is It Still Usable?" *Journal of American Studies*, I (Apr. 1967), 63.

values. This position has had both radical and conservative advocates, but none more explicit than the conservative Conyers Read, who in his presidential address of 1949 enlisted the historian in the cold war. As he saw it, "the first prerequisite of a historian is a sound social philosophy," and "As historians we must carry back into our scrutiny of the past" our political faith. He advocated social controls over historians and demanded that we "accept and endorse such controls as are essential for the preservation of our way of life." They would be, he assured us, "no menace to essential freedoms." This meant that "we recognize certain fundamental values as beyond dispute," values that we "must defend against all assaults, historical or otherwise."⁵³

Those Americans who pursue the usable past have unconsciously assumed a space-time continuum that confuses forebears with descendants and homogenizes time past with time present. Whether they are conservatives with friendly and affectionate feelings toward the past or radicals with cynical and iconoclastic attitudes, the resulting confusion is the same. A fatal betrayal of the craft would be to permit the profession of history to become inextricably entangled with the future of the past, the purposeful past of the rationalizers, the justifiers, and the propagandists. Anyway, it is no good any longer urging upon a chaotic era of discontinuities and disruptions a specious time past continuum—not with the classic American continuity myths of security and invincibility, of success and innocence crumbling about our heads. Without some continuities the social fabric would disintegrate. Many continuities, of course, persist. But ours is essentially an age of disjuncture, not of continuity. Indifference to these conditions and insensitivity to any light that the world of art or science or philosophy may throw upon them would be a disservice to the craft.

"It is not inconceivable," wrote Marc Bloch shortly before his tragic death, that our civilization "may, one day, turn away from history, and historians would do well to reflect upon this possibility."⁵⁴ We have seen that the future of history, its status in the academic world, and its prestige in the intellectual community have been seriously questioned of late. Referring to the future of another branch of learning, Berlin writes, "There exist only two good reasons for the demise of a discipline: one is that its central presuppositions . . . are no longer accepted. . . . The other is that new disciplines have come to perform the work originally undertaken by the older study."⁵⁵ Such was the fate, he points out, that overtook astrology, alchemy, and phrenology, among others, on which their offspring performed a species of parricide. History would not seem destined for that fate.

Marc Bloch had the audacity "to claim for history the indulgence due to all

⁵³ Conyers Read, "The Social Responsibilities of the Historian," *American Historical Review*, LV (Jan. 1950), 284-85.

⁵⁴ Marc Bloch, *The Historian's Craft* (New York, 1964), 5.

⁵⁵ Sir Isaiah Berlin, "Does Political Theory Still Exist?" in *Philosophy, Politics and Society*, ed. Peter Laslett and W. G. Runciman, 2d Ser. (New York, 1962), 1-2.

new ventures." While history has remained "quietly loyal to its glorious Hellenic name," he wrote, it has "grown old in embryo" as myth and legend, chronicle and romantic storytelling, but it is "still very young as a rational attempt at analysis."⁵⁶ In its new phase it bears little more resemblance to the embryo than astronomy to astrology or chemistry to alchemy. Unlike the sciences, however, history did not change its name with its phases. One result is that it has been falsely judged by the antiquity of the old Greek name as well as by association with an antique subject matter. Unlike the sciences also, history has not been periodically endowed with "a common paradigm" that is said to have "freed the scientific community from the need constantly to re-examine its first principles."⁵⁷ Given the nature of the craft, history probably never will be so freed or so endowed. "The uncertainties of our science must not, I think," wrote Bloch, "be hidden from the curiosity of the world. They are our excuse for being." He urged professional historians, "above all the younger ones, to reflect upon these hesitations, these incessant soul-searchings, of our craft."⁵⁸ In the new era of soul-searching for which our guild seems destined, it is well to keep his valiant example in mind.

Exercises in self-doubt and self-criticism are not new to the craft. They need never become self-destructive. After all allowance for its shortcomings, all admission of outrageous pretensions, and all deference to the achievements of other arts and sciences, history may still claim legitimate and vital roles unfilled by other disciplines. These roles multiply rather than diminish in times such as our own, times of striking disjuncture between past and present. For it is in just such times as these that anachronisms proliferate, and when they cease to be harmless myths and grow into rigid dogmas over which nations go to war and races of men tear at each other's throats. Anachronisms are the peculiar concern of historians, and as causes for concern and peril they were never more numerous, more menacing than they are in our time. The historian is peculiarly fitted also to serve as mediator between man's limitations and his aspirations, between his dreams of what ought to be and the limits of what, in the light of what has been, can be. There is no other branch of learning better qualified to mediate between man's daydream of the future and his nightmare of the past, or, for that matter, between his nightmare of the future and his daydream of the past. So long as man remains recognizably human, he will remain a creature with both a past and a future. A creature so long described as earth-bound and so newly transcending those bounds, so giddy over his spectacular innovations, so guilt-ridden about his past, and so anxiety-ridden about the present and the future is not a creature who can safely turn away from history.

⁵⁶ Bloch, *Historian's Craft*, 13.

⁵⁷ Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962), 162.

⁵⁸ Bloch, *Historian's Craft*, 51.

The Advent of Printing in Current Historical Literature: Notes and Comments on an Elusive Transformation

ELIZABETH L. EISENSTEIN

IN the late fifteenth century, book production moved from scriptoria to printers' workshops. This shift, which revolutionized all forms of learning, was particularly important for historical scholarship. Ever since then, historians have been indebted to Gutenberg's invention; print enters their work from start to finish, from consulting card files to reading page proofs. Because historians are usually eager to investigate major changes and this change transformed the conditions of their own craft, one would expect the shift to attract much attention from the profession as a whole. A recent survey of varied studies shows the contrary to be true. It is symbolic that Clio has retained her handwritten scroll. So little has been made of the move into new workshops, that, after five hundred years, the muse of history still remains outside.

"History bears witness to the cataclysmic effect on society of inventions of new media for the transmission of information among persons. The development of writing and later the development of printing are examples. . . ."¹ In so far as flesh-and-blood historians who turn out articles and books actually bear witness to what happened in the past, the effect on society of the development of printing, far from appearing cataclysmic, is remarkably inconspicuous. Many studies of developments during the last five centuries say nothing about it at all.

Those who do touch on the topic usually agree that the use of the invention had far-reaching effects. Francis Bacon's aphorism suggesting that it changed "the appearance and state of the whole world" is cited repeatedly and with approbation. But although many scholars concur with Bacon's opinion, few have tried to follow his advice and "take note of the force, effect, and consequences" of Gutenberg's invention. Much attention is paid to developments that paved the

► Mrs. Eisenstein is an adjunct professor at American University, and her chief interest is in European intellectual history, especially at the time of the French Revolution. She received her Ph.D. at Radcliffe College in 1953, having studied under Crane Brinton. An earlier article, "Who Intervened in 1788? A Commentary on The Coming of the French Revolution" (AHR, LXXI [Oct. 1965], 77-103), has appeared in *The French Revolution: Conflicting Interpretations*, ed. F. A. Kafker and J. M. Laux (New York, 1968). Her most important publication is *The First Professional Revolutionist: Filippo Michele Buonarroti* (Cambridge, Mass., 1959), and she is now working on a study of the historical consequences of the advent of printing. She is currently serving as vice-president of the Society for French Historical Studies.

¹ Review of *The New Media and Education*, ed. P. Rossi and B. Biddle (Chicago, 1967), *American Journal of Sociology*, LXXIII (Sept. 1967), 255.

way for this invention. Many efforts have been made to define just what Gutenberg did "invent," to describe how movable type was first utilized and how the use of the new presses spread. But almost no studies are devoted to the consequences that ensued once printers had begun to ply their new trades throughout Europe.

There is, to be sure, a large and growing literature on the history of printing and related topics.² Although much of it seems to be written by and for specialists—custodians of rare books, experts on typography, bibliographers, literary scholars concerned with press variants, and the like—this literature does contain material of more wide-ranging interest. Historians working in neighboring fields, such as economic history, comparative literature, or Renaissance studies, have also contributed useful treatments of special aspects. The field of social history, however, has yielded the richest harvest. The would-be investigator is confronted by a bewildering abundance of studies on such topics as investment in early presses and the book trade in various regions; labor conditions and social agitation among typographers; the publication policies of certain scholar-printers; censorship, privileges, and copyright laws; special aspects of pamphleteering and journalism; the professional author; and the reading public.³ This list could be extended indefinitely.

Furthermore, several works that synthesize and summarize portions of this large literature have recently appeared. Thus Rudolph Hirsch surveys problems associated with "printing, selling and reading" during the first century after Gutenberg. He has tried (unsuccessfully, in my opinion) to aim his work at "the general reader of social and intellectual history" as well as at the specialist.⁴ A more extensive, well-organized volume, which skillfully covers the first three centuries of printing, has appeared in the "Évolution de l'humanité" series. An even broader coverage, embracing "five hundred years," is provided by S. H. Steinberg's remarkably succinct semipopular English survey.⁵ All three of these books sum-

² Thus over 3,200 titles were listed by Douglas McMurtrie, *The Invention of Printing: A Bibliography* (Chicago, 1942), and the number has grown rapidly in the last quarter of a century. A good recent selective bibliography is in W. T. Berry and H. E. Poole, *Annals of Printing: A Chronological Encyclopaedia from Earliest Times to 1950* (London, 1966), 287-94.

³ The wide range of periodicals containing relevant material is suggestive. Apart from a number of journals specifically devoted to special aspects (such as *The Library* or the *Gutenberg-Jahrbuch*), I have also found useful data in the *Journal des Savants*, the *UCLA Law Review*, *Bibliothèque d'Humanisme et Renaissance*, *Archiv für Reformationsgeschichte*, *Isis*, *Shakespeare Studies*, and other relatively specialized journals.

⁴ Rudolph Hirsch, *Printing, Selling and Reading 1450-1550* (Wiesbaden, 1967). In my view only the specialist will want to consult Hirsch's uneven study. On larger issues, the works (mentioned in note 5, below) by Lucien Febvre and H. J. Martin and by S. H. Steinberg provide better guidance and serve the general reader more successfully. For example, Hirsch has no bibliography; Febvre and Martin have an excellent one. The unfavorable review of Hirsch's work in the *Times Literary Supplement*, Sept. 21, 1967, 848, seems to me to be a better appraisal than the eulogy by Bernard Rosenthal, *Papers of the Bibliographical Society of America*, LXII (No. 1, 1968), 139-44.

⁵ Lucien Febvre and H. J. Martin, *L'apparition du livre* (Paris, 1958). Febvre died before the book was completed, and credit for most of it should go to Martin. A recent notice of a new work, H. J. Martin, *Livre, pouvoirs et société à Paris au XVII^e siècle* (2 vols., Geneva, 1969), describes it as a "splendid sequel." See "Books in France," *Times Literary Supplement*, Nov. 20, 1969, 1344. Two volumes on conditions in seventeenth-century Paris are not really a sequel to one volume covering all

marize data drawn from many scattered studies. But although the broader historical implications of these data are occasionally hinted at, they are never really spelled out. Like the section on printing in *The New Cambridge Modern History*,⁶ which seems to be detached from the rest of the multivolumed series, the contents of these surveys rarely enter into treatments of other aspects of the evolution of humanity.

According to Steinberg, "The history of printing is an integral part of the general history of civilization."⁷ Unfortunately the statement is not applicable to written history as it stands although it probably is true enough of the actual course of human affairs. Far from being integrated into other works, studies dealing with the history of printing are isolated and artificially sealed off from the rest of historical literature. In theory, these studies center on a topic that impinges on many other fields. In fact, they are seldom consulted by scholars who work in any other field, perhaps because their relevance to other fields is still far from clear. "The exact nature of the impact which the invention and spread of printing had on Western civilization remains subject to interpretation even today."⁸ This seems to understate the case; for there are almost no interpretations, however inexact, upon which scholars may draw when pursuing other inquiries. The effects produced by printing have aroused little controversy, not because views on the topic coincide but because almost none have been explicitly set forth. Indeed those who seem to agree that momentous changes were entailed always seem to stop short of telling us just what they were.

Two citations may suffice to illustrate the range of evasive tactics that are employed. The first comes from a justly celebrated study of comparative literature by an eminent literary historian: "The immense and revolutionary change which it [the invention of printing] brought about can be summarized in one sentence: Until that time every book was a manuscript."⁹ The author goes on to discuss scribal book production in a highly romantic and fanciful vein. Nothing more is said about what happened after books ceased being manuscripts; not a single useful clue is offered concerning the nature of the "immense and revolutionary change." That an otherwise careful scholar entertains the notion of summarizing such a change in a single sentence is surely remarkable. The author of the second citation, who has contributed much to the special literature on printing and whose competence in this field gives his views added weight, provides a less exception-

of Europe during three centuries. S. H. Steinberg, *Five Hundred Years of Printing* (rev. ed., Bristol, 1961), covers even more territory in fewer pages than do Febvre and Martin. But Steinberg also offers much that is not duplicated in the other surveys; even where, as in treating the first century of printing, all of them overlap.

⁶ Denys Hay, "Literature: The Printed Book," in *The New Cambridge Modern History*, II, *The Reformation, 1520-1559*, ed. G. R. Elton (Cambridge, Eng., 1958), 359-86. See note 19, below.

⁷ Steinberg, *Five Hundred Years of Printing*, 11.

⁸ Hirsch, *Printing, Selling and Reading*, 2.

⁹ Ernst Curtius, *European Literature and the Latin Middle Ages*, trans. W. R. Trask (Torchbook ed., New York, 1963), 238.

able approach. "It would require an extensive volume to set forth even in outline the far-reaching effects of this invention in every field of human enterprise."¹⁰ This is probably so. Yet no volume, whether slim or extensive, can set forth, or present in outline form, effects that have not yet been described or explicitly defined. Douglas McMurtrie's reference to an immense unwritten volume turns out to be scarcely more edifying than Ernst Curtius' summary sentence. In both instances we learn nothing about seemingly momentous consequences except that they occurred. Nor is the curious reader offered any guidance about where to go to learn more.

Since our concern is with "far-reaching effects" that, by common consent, left no field of human enterprise untouched, we might well wonder why such effects still remain undetermined. "Neither political, constitutional, ecclesiastical, and economic events, nor sociological, philosophical, and literary movements can be fully understood without taking into account the influence the printing press has exerted upon them."¹¹ Generations of scholars have closely scrutinized all these events and movements, with the aim of understanding them more fully. If the printing press exerted some influence upon them, why is this influence so often unnoted, so rarely even hinted at, let alone discussed? The question is worth posing if only to suggest that the effects produced by printing are by no means self-evident. In so far as they may be encountered by scholars exploring different fields, they are apt to pass unrecognized. To track them down and set them forth—in an outline or some other form—is not an easy task.

When McMurtrie and Steinberg refer to the impact of printing on every field of human enterprise, it is not clear just what they have in mind. They seem to be pointing, in part at least, to indirect consequences that have to be inferred and that are associated with the consumption of printed products or with changed mental habits. Such consequences are, of course, of major historical significance and impinge on most forms of human enterprise. Nevertheless, it is difficult to describe them precisely or even to determine exactly what they are. It is one thing to describe how methods of book production changed after the mid-fifteenth century or to estimate rates of increased output; it is another to decide how access to a greater abundance or variety of written records affected ways of learning, thinking, and perceiving. Similarly, it is one thing to show that typography introduced standardization; it is another to decide how laws, languages, or images were affected by more uniform texts. Even at present, despite all the data being obtained from living, responsive subjects, despite all the efforts being made by public opinion analysts, pollsters, or behavioral scientists, we still know little about how access to printed materials affects human behavior. (Recent controversies on the desirability of censoring pornography show how ignorant we are.) Historians who have to reach out beyond the grave to reconstruct past forms of con-

¹⁰ Douglas McMurtrie, *The Book* (Oxford, Eng., 1943), 136.

¹¹ Steinberg, *Five Hundred Years of Printing*, 11.

sciousness are particularly handicapped in dealing with such issues. Theories about unevenly phased changes affecting literacy rates, learning processes, attitudes, and expectations do not lend themselves to simple, clear-cut formulations that can be empirically tested or that can be easily integrated into conventional historical narratives.

Problems posed by some of the more important effects produced by the shift from script to print, by indirect consequences that have to be inferred, and by imponderables that defy accurate measurement probably can never be overcome entirely. But such problems could be confronted more squarely if other impediments did not lie in the way. Among the far-reaching effects that need to be noted are many that still affect present observations and that operate with particularly great force upon every professional scholar. Thus constant access to printed materials is a prerequisite for the practice of the historian's craft. It is difficult to observe processes that enter so intimately into one's own observations. To deal with the consequences of the advent of printing, for example, one has to compare developments that preceded the event with those that came thereafter. But there is no way of observing the conditions of scribal culture except through a veil of print. To reconstruct the age of scribes, historical imagination has to be called into play; yet this particular faculty can only be exerted by turning to printed reference guides and card files, that is, by utilizing the resources of a typographic culture. This recourse, in turn, blurs clear perception of conditions that prevailed before such recourse was possible.

Here again obstacles are encountered that can never be overcome entirely but might be more successfully handled if they were perceived more clearly. When one has been trained to view phenomena at a distance, however, one is prone to myopia about those that occur, so to speak, directly under one's eyes. The apparent blindness of most scholars to effects exerted by the medium they look at every day has been most emphatically stressed and elaborately treated by Marshall McLuhan.¹² According to his thesis, subliminal effects are engendered by repeatedly scanning lines of print presented in a standardized format. Habitual book readers are so subjectively conditioned by these effects that they are incapable of recognizing them. The bizarre typographical format of *The Gutenberg Galaxy* is presumably designed to counteract this conditioning and to jolt the reader out of his accustomed mental ruts. McLuhan attributes his own awareness of and ability to withstand the quasi-hypnotic power of print to the advent of new audio-visual and electronic media. By affecting our senses and conditioning our perception differently, he holds, the new media have begun to break the bookish spell that held literate members of Western society in thrall during the past five centuries. It is noteworthy that McLuhan, while presenting his thesis in an unconventional

¹² Marshall McLuhan, *The Gutenberg Galaxy: The Making of Typographical Man* (Toronto, 1962), *passim*. For further elaboration on the same theme, see *id.*, *Understanding Media: The Extensions of Man* (New York, 1965).

format, tends to undermine it at the same time by drawing heavily for substantiation on conventional scholarly literature. His book is a patchwork of citations that could only have been compiled by spending time in the library scanning lines of print. It should be noted, further, that the scholars who furnish McLuhan with the citations assembled in his book had managed to develop and convey various insights on the topic by entirely conventional means. The chaotic appearance of *The Gutenberg Galaxy* probably owes less to the impact of new media than to the old-fashioned difficulty of trying to organize material gleaned from wide-ranging reading—evaded in this instance by an old-fashioned tactic, by resorting to scissors and paste.

I agree with McLuhan, on the whole, that the more the medium has been used the less conspicuous it has become. This still holds true, and his own work provides a case in point. When he argues that typography has become obsolescent and that an “electric age” has outmoded the “technology of literacy,” he is himself, in my view, at least, failing to take full note of what is under his own eyes and that of the reader he addresses.

Elaborate media analysis does not seem to be required to explain current myopia about the impact of print. Since Gutenberg’s day, printed materials have become exceedingly common. They ceased to be newsworthy several centuries ago and have attracted ever less attention the more ubiquitous they have become. But although calendars, maps, timetables, dictionaries, catalogues, textbooks, and newspapers are taken for granted at present (or even dismissed as old-fashioned by purveyors of novelties), they continue to exert as great an influence on daily life as ever they did before. Indeed the more abundant they have become, the more frequently they are used and the more profound and widespread is their impact. Typography is thus still indispensable to the transmission of the most sophisticated technological skills. It underlies the present explosion in knowledge and, in my opinion, accounts for much that is singled out as peculiarly characteristic of mid-twentieth-century culture. But, I repeat, the more printed materials accumulate, the more we are inclined to overlook them in favor of more recent, less familiar media. Articles speculating about the effects of television will thus find a larger market than conjectures about the impact of print. Because the latter has become increasingly less visible, repercussions that are actually being augmented and amplified at present are paradoxically believed to be diminishing instead.

The prolonged ubiquity of printed materials does not, however, completely explain current myopia. The era of incunabula had ended well before Bacon, Campanella, and Galileo were born. But none of them was inclined to take typography for granted; on the contrary, each commented on its great significance. To be sure, by present standards, printed materials were relatively scarce in the early seventeenth century. By contemporary standards, however, they were re-

markedly abundant and were already being described as a glut on the market.¹³ Since the scanning of printed pages had become a familiar daily routine in seventeenth-century scholarly circles and yet the letterpress was still being discussed as a conspicuous innovation, our present tendency to overlook it needs further explanation.

An additional point is worth considering. In Bacon's day the number of important "inventions unknown to the ancients" was still believed to be relatively small. Bacon himself listed only three when he coupled the compass and gunpowder with movable type. As printed materials have become more abundant, however, unprecedented innovations have also proliferated so that the number of inventions unknown to the ancients is now seen to be much larger than before. Given the cumulative impact of modern technological advance, the Renaissance convention of coupling the printing press with other inventions has resulted in diminishing its prominence. The expansion of the so-called knowledge industry has similarly helped to relegate the fifteenth-century invention to an increasingly marginal position.¹⁴ For even while modern technology has brought forth many new inventions, modern scholarship has uncovered many old ones. The list of those that were unknown to the ancients and yet invented before Bacon's day is becoming longer and longer. With horse collars, grist mills, eyeglasses, and the like being added to already cluttered inventories, the printing press has also become somewhat less distinctive.¹⁵ In view of the oddly assorted company Gutenberg's invention keeps at present (I have found it placed between the insurance contract and advances in metallurgy in one account; between the mechanical clock and the university in another; between double-entry bookkeeping and spectacles in a third),¹⁶ one wonders, indeed, where to look for it. And only specialists with fairly circumscribed interests are inclined to embark on the search.

The steady division of intellectual labor, which has accompanied the expansion of the knowledge industry, has also diminished the number of those who might be interested in following Bacon's advice. First classified as an "invention" by

¹³ See citations from Lope de Vega and Thomas Browne in Preserved Smith, *A History of Modern Culture: The Enlightenment* (2 vols., New York, 1930-34), II, 276. Although Smith implies that it was only in the seventeenth century that the multiplication of books began to be felt as an oppression, Erasmus had already complained about the swarms of new books. (See "Festina Lente," in *The "Adages" of Erasmus: A Study with Translations*, ed. and tr. Margaret Mann Phillips [Cambridge, Eng., 1964], 182-83.) This complaint may well derive from a scribal literary convention. Petrarch, like Juvenal, had, after all, complained about the number of scribblers at work in his day.

¹⁴ Publications by the Society of the History of Technology during the past decade suggest the problem of squeezing Gutenberg's invention onto crowded inventories. References to printing are rare in the articles, reviews, and cumulative bibliographies published in *Technology and Culture*.

¹⁵ I say "somewhat less distinctive" advisedly. Since writing the above I came across a piece of evidence that undermines it. According to Lynn White, jr., *Medieval Technology and Social Change* (rev. ed., Oxford, Eng., 1967), 135, n. 1, a sixteenth-century engraver made printing, the compass, and gunpowder share a plate commemorating postclassical inventions with the stirrup, the mechanical clock, the discovery of America, silk, distilling, and a purported cure for syphilis. Modern inventories are not much more cluttered than this.

¹⁶ Robert Lopez, "Hard Times and Investment in Culture," in W. K. Ferguson et al., *The Renaissance: Six Essays* (New York, 1962), 33; Lewis Mumford, *Technics and Civilization* (New York, 1934), 134-37; Dana B. Durand, *The Vienna-Klosterneberg Map Corpus: A Study in the Transition from Medieval to Modern Science* (Leiden, 1952), 282.

early chroniclers, the topic of printing is now either subdivided among specialists in bibliography, typography, and the like or placed under the heading of the history of technology. If it receives a somewhat cursory treatment from busy historians of technology, it gets even less attention elsewhere. Since sizable investments were required to establish early presses and since early printers were pioneers in new mass production and wholesale marketing techniques, the new mode of book production provides a good example of early capitalist enterprise and, as such, may receive some attention from certain economic historians. For the rest, for historians concerned with political, constitutional, intellectual, or cultural developments the shift from script to print seems to escape notice altogether.

Thus countless standard histories of Western philosophy, religion, and science, of political and economic theory, of historiography, of literature and the fine arts pass over the topic entirely. Not only modern specialization but also the persistence of a venerable philosophical tradition, of proud ignorance concerning material and mechanical phenomena, may help to account for neglect by intellectual and cultural historians. Because of this neglect the history of ideas is weakened as a discipline. When ideas are detached from the media used to transmit them, they are also cut off from the historical circumstances that shape them, and it becomes difficult to perceive the changing context within which they must be viewed. This point is not only pertinent to most histories of Western philosophy or literature; it also applies to most treatments of the history of science and of historiography.¹⁷ The shift from script to print affected methods of record keeping and the flow of information. We cannot, each of us, study all aspects of the past, and intellectual historians may be well advised to leave many inventions, like the stirrup or grist mill, to other specialists. To treat Gutenberg's invention in this way, however, is to miss the chance of understanding the main forces that have shaped the modern mind. The problem of relating intellectual history to the rest of history could also be handled more effectively if greater attention were paid to the impact of print. Attempts to relate ideas to social action, to link the Marxist "superstructure" to actual modes of production, or to develop a "sociology of knowledge" are likely to produce strained and awkward solutions when the communications revolution is not taken into account. Most speculation about mind and society or mentalities and material conditions seems premature and excessively abstract. Before theorizing in general about these issues, one should consider more concretely how specific forms of book learning may be related to specific techniques for producing and distributing books.

The printing press, of course, is not merely classified topically as a technolog-

¹⁷ Some suggestions as to how the advent of printing may have affected historical consciousness and other aspects of Western thought are offered in Elizabeth L. Eisenstein, "Clio and Chronos: Aspects of History-Book Time," *History and Theory* (Suppl. No. 6, 1966), 36-65, and "Some Conjectures about the Impact of Printing on Western Society and Thought," *Journal of Modern History*, XL (Mar. 1968), 1-56. See also John Ziman, *Public Knowledge: The Social Dimension of Science* (Cambridge, Eng., 1968), 45.

ical innovation and assigned to specialists in the history of technology; it is also classified chronologically and is assigned to historians who specialize in the period that coincides with the age of incunabula. As a period piece, it usually appears (often coupled with the fall of Constantinople) in its mid-fifteenth-century location on time charts, coming in for mention under the appropriate chapter heading in general surveys and textbooks. Placed somewhere between the Black Death and the discovery of America in the minds of attentive students, it occupies a relatively inconspicuous position among all the events that are passed in review when thousands of years are surveyed. Most professional historians, specializing in various periods, are even less likely to pause over the advent of printing than those who read introductory surveys. It escapes the attention of all ancient historians and of most medievalists. Nor does it attract much more notice from specialists in the periods that postdate its advent. Discussion of the historical significance and broader social consequences of the shift from script to print is generally left to those who specialize in the problematic era known as the "Renaissance."

The advent of printing does not loom very large in Renaissance studies, however; it often appears as something of an afterthought coming, as it does, well after the beginning of the classical revival in Italy. Although some early philosophers of history, like Condorcet, regarded printing as an epoch-making event and arranged epochal divisions accordingly, the periodization schemes that were fixed later on and entered into our present standard curriculums place the advent of printing at some mid-point in an ill-defined age of transition. As things stand now, the shift from script to print receives much less attention than the hypothetical era within which it is seen to have occurred. Debates that center on the "problem of the Renaissance" seem to leave little room for the establishment of printers' workshops. Thus art historians usually single out central perspective rather than movable type as the most noteworthy Renaissance invention, while intellectual historians tend to merge the activities of scribes and printers, copyists and editors when tracing the humanist movement or the "rise of historical scholarship."¹⁸ In more general works, earlier chapters frequently postpone discussion of Gutenberg's invention until the Renaissance has ended. The shift from script to print is never discussed; the age of incunabula is permitted to close as inconspicuously as it opened; and the subject is covered only under miscellaneous aspects of the Reformation.¹⁹

¹⁸ Erwin Panofsky, *Renaissance and Renascences in Western Art* (Uppsala, 1960), 108, describes central perspective as "the most characteristic invention of the Renaissance." Paul Oskar Kristeller, *Renaissance Thought: The Classic Scholastic and Humanist Strains* (New York, 1961), 14, like many other Renaissance scholars, does not differentiate between the activities of humanists "as copyists" and "later as editors." For a detailed discussion of these references and many others, see Elizabeth L. Eisenstein, "The Advent of Printing and the Problem of the Renaissance," *Past and Present* (Nov. 1969), 19-89.

¹⁹ *The New Cambridge Modern History* places the discussion of printing in its second volume: "The Reformation (1520-1559)," where it appears as a subsection of a chapter on "intellectual tendencies." (See note 6, above.) Since the shift in book production occurred in the fifteenth century, I

When the innovation is placed, more accurately, in the fifteenth century, it is usually mentioned in a casual manner and treated as an incidental example of some other concurrent development, if not to describe an early capitalist enterprise, then to illustrate the expansion of a literate laity, to demonstrate late medieval technological advances, or to show how much Westerners relied on the importation of Far Eastern techniques. George Sarton has, nevertheless, described movable type as "the greatest invention of the Renaissance" and, in a symposium devoted to the era, has tried, however briefly, to spell out some of its consequences.²⁰ Myron Gilmore also devotes several paragraphs to the topic in his volume in "The Rise of Modern Europe" series. When we consider the treatments of specialists in the periods that postdate the age of incunabula, a Renaissance scholar such as Gilmore will appear to be farsighted indeed.

The invention and development of printing with movable types brought about the most radical transformation in the conditions of intellectual life in the history of western civilization. It opened new horizons in education and in the communication of ideas. Its effects were sooner or later felt in every department of human activity.²¹

Although he thus points toward later repercussions, the chronological limits of Gilmore's volume prevent his describing them in any detail. Subsequent volumes in the series that might be expected to describe somewhat more fully effects that were eventually "felt in every department of human activity" contain no explicit reference to these effects. This tendency to curtail discussion of a continuing transformation at the very point, in the early sixteenth century, when it was just beginning to gather momentum is unfortunately typical. Studies devoted to the centuries that followed the era of incunabula relegate consideration of printing to a variety of fringe areas and minor subspecialties. Separate marginal plots within the large, somewhat amorphous, field of social history are cultivated by authors of monographs devoted to the book trade, to patronage and censorship, to belles-lettres, journalism, and education, to public opinion and propaganda analysis, or to the internal organization of printing industries in diverse regions. Except for occasional references to the "rise" of the "reading public" and the emergence of "professional" authors in the eighteenth century, to the role of the "press" and of "public opinion" in the nineteenth century, one might conclude from most current history books that the social and intellectual transformations introduced by printing had petered out with the last Reformation broadside. That the new presses disseminated Protestant views is probably the only aspect of their impact upon Western civilization with which most historians

think printing should be introduced in the first volume, where it could precede and set the stage for a chapter by R. Weiss, "Learning and Education in Western Europe from 1470 to 1520," *The New Cambridge Modern History*, I, *The Renaissance, 1493-1520*, ed. G. R. Potter (Cambridge, Eng., 1957), 95-126.

²⁰ George Sarton, "The Quest for Truth: Scientific Progress during the Renaissance," in Ferguson *et al.*, *Renaissance*, 67.

²¹ Myron Gilmore, *The World of Humanism 1453-1517* (New York, 1952), 186.

of modern Europe are familiar. With regard to the Lutheran revolt, moreover, the effects produced by printing have been drastically curtailed and restricted to the single function of "spreading" ideas. The possibility that the new technology posed new issues for churchmen and (by duplicating Bibles, for example) contributed to shaping the Protestant movement as well as to its subsequent spread is not raised.²² In any case, once chapters devoted to the Reformation are closed and the spread of Protestantism has become a *fait accompli*, Gutenberg's invention recedes into oblivion. The spotlight of history is focused on later, seemingly more significant developments.

Among historians dealing with the post-Reformation era, the invisibility of the cumulative impact exerted by the advent of printing is particularly marked. The intellectual and political revolutions of the seventeenth and eighteenth centuries are placed within the context not of a postprint but rather of a pre-industrialized society.²³ In attempting to explain these revolutions, shifts associated with trade routes and prices, land use and crops, and status groups and classes are discussed at length. Changes affecting the format of maps, charts, and tables, of law books and reference works, calendars and treaties, bills and petitions rarely enter the discussion. We hear much about the effects of the commercial revolution but nothing about those of the communications revolution.

Like the rapidly developing bibliography on the history of technology, a long-lived, ever-growing literature on the Industrial Revolution has also helped to distract attention from the invention Bacon once regarded as conspicuous. Ever since Marx inverted Hegel's historical grand design, historians have been preoccupied with the mechanization of many modes of production. But they have curiously omitted from their accounts the momentous shift from handwriting to type casting. In general works, for example, more emphasis is placed on shifts affecting textile production and the cloth trade than on those affecting book production and the book trade. How the advent of printing affected commerce and industry, in general, and how it revolutionized methods of advertising and systems of currency, in particular, go unnoted in most surveys of economic history. The last two centuries have seen too many changes affecting agriculture, industry, and transportation for the later phases of the continuing communications revolution—the advent of new paper mills, the use of iron machines and steam presses—to receive more than cursory notice. Even the earliest phases are now overshadowed by the cluster of changes that came three centuries later; so

²² Some of the ways in which printing may have contributed to the Reformation are noted in Eisenstein, "Some Conjectures," 34–35. See also Lawrence Stone, "Literacy and Education in England 1640–1900," *Past and Present* (Feb. 1969), 78–79.

²³ An exception that is too recent to have been assimilated in the literature on the French Revolution is the collaborative volume sponsored by the *Sixième Section* of the *École Pratique des Hautes Études*, *Livre et société dans la France du XVIII^e siècle* [ed. François Furet] (The Hague, 1967). Other pertinent references to recent studies are given by Louis Trénard, "L'histoire des mentalités collectives: Les livres, Bilan et perspectives," *Revue d'histoire moderne et contemporaine*, XV (Oct.–Dec. 1968), 691–703; and by Stone, "Literacy and Education." But, in my view, even these new studies tend to muffle the impact of the typographical revolution.

that the advent of printing, instead of being treated as an event that is *sui generis* and that should be examined on its own terms, is made to serve merely as another precursor of later techniques of mass production. Where it is not mentioned as one among many late medieval technological innovations, indeed, printing is usually cited as one more example of budding capitalistic enterprise. Coupled with sixteenth-century mining and shipbuilding, instead of compass and gunpowder, it is relegated to the position of forerunner and made to anticipate later large-scale industrial enterprises. The extent to which the “invention of a method of invention” and the Industrial Revolution itself depended on an earlier transformation that continued to gather momentum is rarely if ever discussed.

As one among numerous inventions, the “divine art” has become less illustrious. As a revolutionary process that helped to usher in modern times, the shift from script to print is overshadowed by later great transformations and placed by periodization schemes into a closed chapter. Still historians are presumably well equipped to open closed chapters. It is, after all, their main stock in trade. The contents of this particular chapter are, however, curiously difficult to decipher. For when we stay in the brief interval allotted to typography in most texts and surveys and try to focus the spotlight more narrowly on its advent, we find that the more closely we look, the less certain we become about what it is that we are called upon to examine. The enigmatic phrase “movable metal type” appears, like the grin of the vanished Cheshire cat, to be all that is left of the first of Bacon’s most conspicuous inventions.

Not only have lists grown longer and boundaries between specialties more formidable; our analyses of “inventions and discoveries” have also become increasingly sophisticated in a way that tends to muffle our sense of their forcefulness and effectiveness. We are more aware than were Bacon and his contemporaries that major innovations do not spring into existence abruptly—as did Minerva from Jove’s brow. They are now regarded less as single events than as complex social processes, representing in turn the end products of other vaster social changes. To account for the utilization of movable type, it is no longer sufficient merely to go to Gutenberg’s workshop in Mainz, or even to argue that one should first go to Laurens Janszoon Coster’s in Haarlem, or possibly also to examine the business operations of Johannes Fust and Peter Schöffer. Instead one must investigate the prior expansion of a literate laity and a manuscript book trade, account for the accumulation of capital required for investment in early plants, or try to explain why printing industries expanded so rapidly in Western Europe during the late fifteenth century and why the invention of movable type did not have similar consequences in the Far East. Furthermore, the first presses now appear merely as the end products of many other innovations drawn from home and abroad. Changes affecting all manner of industries, arts, and crafts—ranging from wine making, cheese making, seal cutting, oil painting, and card playing to

metallurgy and textile production—have to be taken into account. Together with the prior establishment of paper mills and the prior production of block-printed books, such innovations have been examined in sufficient detail to fragment our concept of “printing” as an invention. “It requires a long treatise to say what actually constituted Gutenberg’s ‘invention.’”²⁴ Although we locate the “printing press” on our time charts in the 1450’s, it rests there as a convenient abstraction, as a summary statement of concrete particulars that must, for the most part, be located elsewhere.

To describe the “force and effect” of such an abstraction leads to difficulties. On the one hand, it seems to have changed nothing; on the other, it appears to have transformed everything. Almost all historians agree that no sharp line should be drawn dividing the first half of the fifteenth century from the second. All of them concur with the following judgment. “. . . it was not the production of books that was revolutionized by the use of movable types or its application to the machine-made edition. In fact, printed books were at first hardly distinguishable from manuscripts. . . .”²⁵ Yet they also usually note how Gutenberg “introduced into Europe, more than three centuries ahead of its general adoption by industry, the ‘theory of interchangeable parts’ which is the basis of all modern mass-manufacturing technique.”²⁶ Possibly a sharp line should be drawn severing the last part of the fifteenth century from preceding eras. By 1480 “the basic difference between the effects created by the metal worker and those produced by the quill driver . . . brought about the victory of the punch cutter over the scribe and with it the supersession of the imitation manuscript by the authentic book.”²⁷ In the end even sophisticated moderns fall back on ancient mythology: “Again and again the historian is struck by the fact that . . . various offshoots of Gutenberg’s art sprang into existence full-grown and armed like Athena from Zeus’ forehead.”²⁸

As these passages suggest, it is difficult to deal with the advent of typography without skewing perspectives by resorting simultaneously to two incompatible models of change: the one, gradual and evolutionary; the other, abrupt and revolutionary. There are cogent arguments for regarding Gutenberg’s invention as part of a continuously unfolding process, for presenting it, as Febvre and Martin do, as one element in a larger “ensemble” of transformations. Thus, the invention and utilization of movable type may be viewed as one by-product of previous developments, such as the spread of lay literacy, and as a factor, which, in turn, helped to pave the way for later developments, such as modern mass literacy. Printers and scribes copied each others’ products for several decades and duplicated the same texts for the same markets during the age of incunabula. If we scan the new booksellers’ catalogues in search of new titles reflecting new

²⁴ Steinberg, *Five Hundred Years of Printing*, 23.

²⁵ *Ibid.*, 22.

²⁶ *Ibid.*, 25.

²⁷ *Ibid.*, 29.

²⁸ *Ibid.*, 133.

cultural trends or striking departures in styles of thought and expression, we will be disappointed. During the first century of printing the bookish culture of Western Europe was not very different from that which had prevailed during the last century of scribes. One must wait until a full century after Gutenberg before the outlines of any new world pictures begin to emerge. It seems plausible, in the light of such considerations, to favor a gradualist, evolutionary approach. But there are also compelling reasons for regarding the shift from script to print as a large "ensemble" of changes in itself, for stressing the unprecedented features that typography introduced, and for sharply contrasting the talents that were mobilized and the functions that were performed in scriptoria on the one hand as against the new workshops on the other. As various studies show, historical imagination is required to bridge the gap between the age of the scribe and that of the printer.²⁹ But before it can be bridged, the gap must be acknowledged, and this acknowledgment, in turn, implies acceptance of discontinuity. A persuasive case, then, can also be made for regarding the age of incunabula as a major historical great divide and for viewing the advent of printing as a revolutionary "change of phase."

Although I think this second line of approach is probably the most fruitful one, there seems to be a general reluctance to adopt it. Whenever possible, discontinuities are glossed over; significant distinctions between the two modes of production are not fully spelled out; and various implications of the shift from one to the other are overlooked. At the same time, however, the alternative approach is not consistently employed; in other words, evolutionary gradualism is only intermittently invoked. One must, indeed, read between the lines to determine which model, if any, is being used since all interpretations are indirectly and obliquely conveyed. On a few rare occasions when a relevant question is posed it is apt to be begged or blandly dismissed.

The road from manuscript to print was continuous *and* broken and I venture to say that all great discoveries, all so-called new movements, harbor the same contrasting elements, continuity and radical change. This dichotomy accompanied Humanism, Renaissance, nationalism, capitalism, the Reformation and . . . the splitting of atoms. . . . E. F. Jacob rightly called the XVth century a "remarkable admixture of the old and the new. . . ."

His characterization fits the XVth-century book as well. Old elements remained unchanged, others were transformed, new techniques were developed and the uses of books changed.³⁰

It is difficult enough to grapple with problems raised by the advent of printing without also having to consider the possible relevance of assorted movements that range from humanism to atom splitting. It is disappointing, furthermore,

²⁹ See Pierce Butler, *The Origin of Printing in Europe* (Chicago, 1940), Chap. 1; H. J. Chaytor, *From Script to Print: An Introduction to Medieval Literature* (Cambridge, Eng., 1945); and E. P. Goldschmidt, *Medieval Texts and Their First Appearance in Print* (London, 1943).

³⁰ Hirsch, *Printing, Selling and Reading*, 2.

when one hopes for clarification to be told that all movements contain elements of continuity and radical change and that a given century is an admixture of old and of new. To be reminded of eternal verities is rarely helpful in dealing with specific historical problems. In this instance, I think, obfuscation rather than clarification results. By mentioning a variety of isms and innovations and by applying the same truisms to them all, however, the author does manage to make the unprecedented seem ordinary, the exceptional event appear banal. It is suggestive that the passage ends by deflecting attention away from the wider implications of the problem and toward a more limited range of issues. The general reader recedes, and the bibliophile comes to the fore; the impact of print on the fifteenth-century book rather than on European society is discussed in the paragraphs that follow the citation. Even when the author deals with this narrower topic and brings his special competence to bear on it, his approach is sufficiently ambiguous to leave open the question of how abrupt or gradual, major or minor were the changes typography entailed.

L'apparition du livre compares favorably with Hirsch's work in other respects, but it is no less ambiguous about the question of which model to adopt. The title of the volume suggests that the issue will be handled, if at all, somewhat obliquely. Although the work is actually devoted to "l'apparition du livre imprimé" (as Marcel Thomas tells us on the first page), the reference to print has been dropped from the title. Febvre's preface stresses the larger ensemble of transformations within which Gutenberg's invention should be viewed and also presents the new technology as a mere prologue to later and greater transformations. The first chapter is devoted to the prior advent of paper and to tracing an evolutionary pattern. It describes the gradual expansion of both manuscript book production and of a cosmopolitan lay reading public. The same pattern reoccurs thereafter: divisions between manuscripts and incunabula are blurred, and the lack of important changes in book production techniques during the next three centuries is stressed.³¹ We are also told that the new presses, by duplicating outmoded scribal works more efficiently, did nothing to speed up the adoption of new theories or knowledge and, on the contrary, contributed to cultural inertia.³² In certain scattered passages, however, the typographical industry is "very rapidly modernized"; "medieval" workshops are transformed into "modern plants" as early as the 1480's; a "bouleversement" of literate Europe is produced by the immensely increased output of books during the age of incunabula; the concern of sixteenth-century printers to attract the greatest number of buyers to their wares is described as a step toward "mass culture"; in mid-sixteenth-century Lutheran regions a

³¹ See preface (by Febvre), xxiii-xxix, introd. (by Thomas), 1-24, and Chap. 1 in Febvre and Martin, *Apparition du livre*. In contrast to Steinberg's treatment, evolutionary as opposed to revolutionary changes in book format are stressed by Febvre and Martin, *Apparition du livre*, 108.

³² *Ibid.*, 420-21. I regard this view as mistaken and believe an increased output of old texts contributed to the shaping of new ideas. (See Eisenstein, "Some Conjectures," 8.)

"mass literature" "directed at everyone and accessible to everyone" had already been achieved.³³

It is noteworthy that the scattered passages that hint at remarkably rapid modernization and "bouleversement" are at odds with the general tenor of the work. In so far as Febvre and Martin tend to minimize discontinuous and revolutionary implications and stress the themes of gradualism and continuity, they appear to be conforming to deep-rooted historical convention. Indeed, Denys Hay seems to speak for the entire historical profession when, even while noting the exceptional character of the event, he warns against overdramatizing its consequences.

Some inventions . . . have taken centuries to be widely adopted and even more have taken several generations. Printing was an exception. It spread at a phenomenal speed from Mainz and by the 1490s each of the major states had one important publishing centre and some had several.

. . . it is impossible to exaggerate the rapidity of the transformation. It is all too easy to exaggerate the consequences. . . .³⁴

Some of Hay's readers may need to be cautioned in this way, but his scholarly colleagues certainly do not. For various reasons that cannot be elaborated here, professional historians find it much easier to understate than to exaggerate the consequences. More often than not, as previous remarks suggest, they find it easiest of all to overlook them altogether.

Where historians are prone to be overcautious, others are encouraged to be overbold. Evasion on the part of careful scholars has, by default, allowed the topic to fall into more careless hands. The fifteenth-century "media revolution" is also of interest to those who cultivate various *avant-garde* fields (communications theory, media analysis, and the like) and who scrutinize the current scene without paying much heed to the past. Nonhistorians of this sort, however, are almost certain to go astray if they try to thread their way unaided through five centuries of unevenly phased change. It is not surprising that they become impatient with the absence of clear guidelines and try to take short cuts on their own. In *The Gutenberg Galaxy* McLuhan provides a good case in point. The author has solved his difficulties by the simple, albeit inelegant, device of dispensing with chronological sequence and historical context altogether. Far from appearing to be concerned about preserving proportion and perspective, he impatiently brushes aside all such concerns as obsolete. Developments that spanned the course of five hundred years, affecting different regions and penetrating to different social strata at different intervals are randomly intermingled and treated as a single event—most appropriately described, perhaps, as a "happening."

³³ Febvre and Martin, *Apparition du livre*, 193, 377, 394, 443.

³⁴ Denys Hay, "Introduction," *Printing and the Mind of Man: The Impact of Print on the Evolution of Western Civilization during Five Centuries*, ed. John Carter and Percy Muir (Cambridge, Eng., 1967), xxii. The subtitle of this work is misleading. It is an enlarged descriptive catalogue containing over four hundred entries on early editions of great books displayed at a London exhibition in 1963.

According to some critics and the author himself, the jumbling of data and disregard for sequence are deliberate. *The Gutenberg Galaxy*, we are told in the *Times Literary Supplement*, is an "anti-book." The author has set out to subvert "traditional modes of philosophic-historical argument" and to persuade his readers "that books—a linear progression of phonetic units reproduced by movable type—are no longer to be trusted."³⁵ It seems unlikely that readers of the *Times Literary Supplement* (or of *The Gutenberg Galaxy* for that matter) are in much need of persuasion on this point. Few of them have ever put much trust in books per se; most of them are trained to approach all publications with caution and are inclined toward disbelief when presented with arguments (whatever the format) that are not solidly substantiated.

If one sets out to subvert traditional historical guidelines, almost any other topic would provide a better target than the shift from script to print. Conventional treatments already fall short of encompassing this topic. They need to be extended rather than undermined. Indeed, it is not really accurate to say that McLuhan has taken data out of context since an adequate context has not yet been supplied. The author has, in my opinion, shirked the difficult task of organizing his material coherently. His insistence that coherence is itself outdated is not only an invalid thesis but also a self-serving one. His work does serve a useful function, nevertheless, by pointing to a large number of significant issues that cry out for historical investigation and have, as yet, received almost none.

Perhaps historians are too often discouraged in advance by being reminded repeatedly of the magnitude of the task:

The cumulative effect of the continuing revolution wrought in every aspect of human thought and activity by the invention associated with the city of Mainz is too immense ever to be fully describable. Its consequences to religion, politics and industry are far too vast for assessment by available historians and bibliographers or by any assemblage of scholars to be foreseen at present.³⁶

The prospect of taking on a subject that is "far too vast" to be assessed by any present or future assemblage is likely to daunt even the most audacious individual. If it is too vast to be handled by any single scholar, however, it is, by the same token, also too vast to be avoided by any single scholar. Because of its almost limitless dimensions, the cumulative effect of the "continuing revolution" is bound to impinge, one way or another, on all fields of inquiry, even highly specialized ones. Hence, individual specialists, who are careful and cautious about their work, must, sooner or later, come to terms with it. Enough warnings have already been issued. Historians scarcely need to be alerted to the difficulties of following Bacon's advice. But the importance of trying to surmount these difficulties does need to be stressed. Although the task may never be completed, it should, at least, be begun.

³⁵ "Battle of the Senses," *Times Literary Supplement*, Mar. 1, 1963, 156.

³⁶ Stanley Morison, "The Learned Press as an Institution," *Bibliotheca Docet: Festgabe für Carl Wehmer*, ed. Sigfried Joost (Amsterdam, 1963), 153.

At the Borderland of Law and Economic History: The Contributions of Willard Hurst

HARRY N. SCHEIBER

BOTH historians and students of law have long engaged in analysis of how legal systems are interrelated with the processes of social and economic change. In the literature of American economic history, there is a long tradition of scholarship on the subject of "government and the economy," dating from Guy Callender's writings of fifty years ago. The historians have sought to establish how public policy decisions have affected institutional and economic development and also to assess the decision-making process itself. In the last two decades such scholars as Paul Gates, Oscar and Mary Handlin, Carter Goodrich, and Louis Hartz have given special attention to the study of policy-making processes and effects at the state level in the early nineteenth century.¹ In the field of law, since the revolution in jurisprudence led by Oliver Wendell Holmes and Roscoe Pound early in this century, there has been a conscious effort to uncover the realities of the legal process—to determine how legal systems respond to pressures from the economic market place and from society at large, to assess how formal legal doctrines and rules have been forged in relation to the social milieu and contending real interests, and to discover the varying patterns, in different social contexts and over time, of the interrelationships of law and social change.²

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¹ Guy S. Callender, "The Early Transportation and Banking Enterprises of the States in Relation to the Growth of Corporations," *Quarterly Journal of Economics*, XVII (Nov. 1902), 111-62; Paul W. Gates, *The Illinois Central Railroad and Its Colonization Work* (Cambridge, Mass., 1935), and his numerous studies of land and timber policies; Carter Goodrich, *Government Promotion of Canals and Railroads in the United States, 1800-1890* (New York, 1960), a synthesis, citing in full the studies by the Handlins, Milton Heath, and others. James Willard Hurst, *Law and the Conditions of Freedom in the Nineteenth Century United States* (Madison, Wis., 1956), is a pioneering effort to blend the data and analytic concerns of legal history with the more traditional public policy studies. After the publication of this volume Hurst seldom used his first name and wrote as Willard Hurst.

² A methodological introduction is provided by Willard Hurst in his essay, "Perspectives upon Research into the Legal Order," *Wisconsin Law Review*, 1961 Vol. (May 1961), 356-67; and his ideas are expounded in a different context in his *Justice Holmes on Legal History* (New York, 1964). Important statements on the need for a broadening of traditional concerns in legal history include Daniel J. Boorstin, "Tradition and Method in Legal History," *Harvard Law Review*, LIV (Jan. 1941), 424-36; and Stanley Katz, "Looking Backward: The Early History of American Law," *University of Chicago Law Review*, XXXIII (Summer 1966), 867-84. Representative examples of the new mode of legal history include Leonard W. Levy, *The Law of the Commonwealth and Chief Justice Shaw* (Cambridge, Mass., 1957); Lawrence M. Friedman, *Contract Law in America: A Social and Economic Case*

The subject defines the interactions between law and other systems or institutions, and, therefore, its study naturally occupies a borderland area between formal scholarly disciplines. To define the legal order as something apart from the other segments of social order and to trace causal interconnections between law and the larger society is a demanding task; its full dimensions and difficulties are perhaps nowhere better revealed than in the complex schema set forth long ago by Max Weber.³ Little wonder, then, that when Justice Holmes argued the need to study the origins of formal rules and doctrines in their full historical context, he relied upon an especially terrifying metaphor: the legal historian, he warned, must first "get the dragon out of his cave onto the plain and in the daylight, [where] you can count his teeth and claws, and see just what is his strength. But to get him out is only the first step. The next is either to kill him, or to make him a useful animal."⁴

No scholar of our day has responded to Holmes's challenge so successfully as Willard Hurst. A graduate of the Harvard Law School, later clerk to Justice Louis Brandeis, and now professor of law at the University of Wisconsin, Hurst has written extensively on the history of the American legal system. In his *Growth of American Law: The Law Makers*, published in 1950, Hurst provided a brilliant overview of the history of legal institutions in the United States. But in his later studies, he made a sharp break with the traditional premises of formal constitutional-legal history. In two of his major studies, *Law and the Conditions of Freedom in the Nineteenth Century United States* (1956) and *Law and Social Process in United States History* (1960), Hurst explicitly departs from the view that federal Supreme Court cases are an accurate historical index of the principal points of conflict in the legal system. One finds no assumption, either, that all doctrines pronounced by the Supreme Court were necessarily enforced in the states. Hurst seeks, instead, to define the law as a working system. Exhaustive research is devoted to classification of the business of the courts at the trial and appellate levels in the states, especially in Wisconsin. Hurst has sought to distill the "guiding principles" of the law, moreover, from a variety of evidence left by politicians as well as lawyers and judges; he has attempted as well to read the inarticulate premises of men and organizations that made demands on the legal system; and he has been explicitly concerned with how both the law and legal values conditioned the actions of men in the society at large. In short, Hurst has sought to deal with the impact of law on informal behavior, not alone with the focused demands on the legal system. In his massive study of *Law and Economic*

Study (Madison, Wis., 1965); Robert S. Hunt, *Law and Locomotives: The Impact of the Railroad on Wisconsin Law in the Nineteenth Century* (Madison, Wis., 1958); Samuel Mermin, *Jurisprudence and Statecraft: The Wisconsin Development Authority and Its Implications* (Madison, Wis., 1963); and the works of Willard Hurst, cited below.

³ Max Weber on *Law in Economy and Society*, ed. Max Rheinstein (Cambridge, Mass., 1954).

⁴ Oliver Wendell Holmes, "The Path of the Law" (1897), reprinted in *Landmarks of Law*, ed. Ray D. Henson (New York, 1960), 50.

Growth (1964), a rich analysis of law and the Wisconsin lumber industry from 1836 to 1915, Hurst has provided a case study that shows how a functional treatment of the law (in this case, the treatment of one industry) can bring to light historical change in legal doctrines and also the diverse effects of the law on society. Hurst has not engaged in what Holmes termed a sterile "striving for a useless quintessence of all systems, instead of an accurate anatomy of one."⁵ For in Hurst's works, there is a unique mastery of the concrete particulars of historical "context" and "environment." Yet Hurst has also derived from the data of this nation's experience a set of generalizations about law and social changes, with major ramifications for jurisprudential theory.⁶

In this article I shall seek, first, to abstract the essence of Hurst's historical reconstruction, or model, of how the legal process actually functioned during the nineteenth century. Second, I shall indicate some of the value judgments that underlie Hurst's critique of the legal order of the nineteenth century.⁷ Finally, I shall venture a critique of his historical model, suggesting a somewhat different view of legal and policy processes in American history prior to 1900.

Let us consider at the outset Hurst's historical model of how the legal process actually functioned.⁸ On the input side, Hurst first defines the major variables that affected policy-making style and function. Of overarching importance is the *Volksgeist* of the nineteenth century—the full range of shared assumptions in the society and, as Holmes put it, of the "instinctive preferences and inarticulate convictions" that underlay what men expected of their lawmakers.⁹ A prime place is assigned in Hurst's model to what he terms the widespread popular "faith in the beneficent dynamics of increased productivity." Nineteenth-century America was a society which "expect[ed] substantial change as the norm, which [was] not shocked or afraid of this reality, and which expect[ed] its legal order to take the reality in stride."¹⁰ The "common belief" among Americans, he writes, "the article of faith which made almost all men in this country (save Henry Thoreau)

⁵ *Ibid.*, 55. Hurst, writes the historian David H. Flaherty, "has created an entirely new school of legal historians by taking a very broad approach. . . . Hurst has made all of us conscious of the need to move beyond the traditional framework in the study of the history of American law." (David H. Flaherty, "An Introduction to Early American Legal History," *Essays in the History of Early American Law*, ed. *id.* [Chapel Hill, N. C., 1969], 20, 38.)

⁶ See Earl F. Murphy, "The Jurisprudence of Legal History: Willard Hurst as Legal Historian," *New York University Law Review*, XXXIX (Nov. 1964), 900-43.

⁷ Admittedly, separating the purely historical analysis from jurisprudential concerns in Hurst's studies does risk fragmenting a coherent, unitary scheme of analysis. Yet this is necessary in order to deal with Hurst's contributions to jurisprudence.

⁸ Hurst's definition of legal process includes nearly the full range of phenomena that political scientists commonly subsume when they refer to the "political system." Though Hurst gives relatively little attention to the roles of political parties, he is concerned with the articulation of interest, communications, leadership roles, and other features of political systems. But the dynamic process on which he mainly concentrates is that of policy making, or decision making.

⁹ Oliver Wendell Holmes, *The Common Law*, ed. M. DeW. Howe (Boston, 1963), 32.

¹⁰ Willard Hurst, *Law and Social Process in United States History* (Ann Arbor, Mich., 1960), 236, 26, and *Law and Economic Growth: The Legal History of the Lumber Industry in Wisconsin, 1836-1915* (Cambridge, Mass., 1964), 106-107.

Hamiltonians or Whigs,” was “that it was common sense, and it was good, to use law to multiply the productive power of the economy.”¹¹ The pervasive view of legal and policy processes in American history prior to 1900.

to help shape an environment which gave men more liberty by increasing the practical range of choices open to them and minimizing the limiting force of circumstances.”¹²

In addition to identifying broadly shared basic values, Hurst seeks to define popular views of various specific policy problems. Hurst asserts, for example, that the people of Wisconsin “were busy with private business, from which public affairs were an annoying distraction,” and so they were not inclined to conceptualize taxation as an instrumental policy which might be made part of “a rational . . . program in the interest of the general economy.”¹³ Nor did they exert any pressure on lawmakers to develop a coherent general policy for timberlands or mineral resources. Men commonly regarded these resources as almost inexhaustible; therefore, they accepted fee simple landownership uncritically, without consideration of its long-term social costs. Similarly, their view of property, contract, and torts was predicated on a faith in short-term productivity, on a belief that law should provide mechanisms for releasing private energies, for mobilizing scarce capital, and for devolving resources on private interests toward the goal of maximizing growth.¹⁴

Also on the input side were the specific pressures from society and the economic market place—the well focused demands articulated by what Stewart Macaulay has called the consumers of law. Hurst portrays both the Wisconsin community and the larger American society as communities of expectant capitalists. On specific policy issues that evoked action by “a powerful and dissatisfied interest group” or by several contending interests (regional, functional, or class-oriented), there was sometimes debate that led to consideration of the larger public interest.¹⁵ But, on the whole, special interest demands were either so narrow as to arouse little controversy (and therefore little extension of men’s perception), or so well geared to the prevailing philosophy favoring “productivity” as to win a consensus approval.

The inputs in the policy process as they arose from men’s demands of law were not all self-interested or characterized by a “bastard pragmatism” that subordinated a long-run calculus of costs and benefits to a “short-term, limited-factor” definition of policy goals. For as Hurst asserts, “this society [also] sought constitutional order, which meant order continuously responsible.” This, too, was in a sense pragmatic: just as men accepted change as a natural (and desirable) condition, they wanted their government to reserve its power to institute “reason-

¹¹ *Ibid.*, 171–72, 203.

¹² Hurst, *Law and the Conditions of Freedom*, 6.

¹³ *Id.*, *Law and Economic Growth*, 518; cf. *ibid.*, 220, 597–601.

¹⁴ Hurst, *Law and Social Process*, 79, and *Law and Economic Growth*, 98–108, 461–65.

¹⁵ *Ibid.*, 518; see also *ibid.*, 270–81.

able adjustment of values and procedures" when change altered material circumstances or new experience altered perceptions. "Manifestly," Hurst writes, "our society wanted to keep its hands on its destiny."¹⁶

This faith in a constitutional order required that government itself be accountable, even while it was leaving to the market the exercise of important social functions. This faith also underlay the vitality of police power, as the nineteenth-century courts reasserted government's reserved right to regulate for the common weal.¹⁷ In a word, when "irrevocable" policy choices were manifest, as when the police power was explicitly tested or implicitly threatened, men wanted to avoid irrevocable acts. Opting instead for short-range, incremental decisions was entirely consistent with a pragmatic turn of mind. As Hurst repeatedly underlines, however, many basic policy choices, including such truly irrevocable decisions as allowing Wisconsin's timberlands to be stripped bare, were in fact made by default and without significant debate, because bastard pragmatism kept real long-range costs from the threshold of consciousness.¹⁸

The remaining policy-process inputs in Hurst's model were physical environment and resources, such as the abundance of natural endowments, the scarcity of liquid cash and long-term investment capital, and the quick pace of material change. There was, in addition, the dynamic effect of "drift and cumulation"—institutions and attitudes surviving because they existed, laws perpetuated because they were not intelligently reappraised, and popular attitudes frozen in fixed postures despite changes of circumstances.¹⁹

Of primary importance, in Hurst's model of the policy process itself, is the fact that government was "underdeveloped." Fact-finding was not aided by supportive agencies of the legislatures or the executive branch of government, and *expertise* was lacking. The failure to develop bureaucracies deprived state government of the advantages and insight that it might have derived from greater continuity of operations. Despite the sustaining of the police power by the judiciary, Hurst declares, the courts generally shared the community bias for productivity. Judicial power was constrained in any case, because the initiatives in lawmaking resided with the legislature, and judges consistently validated the legislatures' promotional policies and devolutions of power and function to the private sector. Lumber industry litigation, Hurst finds, offers no "forerunner of the Brandeis brief," for lawyers, like judges, failed to transcend the mundane business of applying policy; they made no effort to examine and redefine funda-

¹⁶ *Ibid.*, 204. On "bastard pragmatism" and antipathy toward application of theory, see Hurst, *Law and Social Process*, 238–39 *et passim*, and "Perspectives upon Research," 349–50.

¹⁷ *Id.*, *Law and Social Process*, 165; see also *ibid.*, 146–67; on the police powers, see Hurst, *Law and Economic Growth*, 204, 760, n. 258.

¹⁸ *Ibid.*, 262–63; Hurst, *Law and Social Process*, 165–67, and "The Law in United States History," *Proceedings of the American Philosophical Society*, CIV (Oct. 1960), 524.

¹⁹ *Id.*, *Law and Social Process*, 23, 63, and *Law and Economic Growth*, 47–52, 117–25, 467; see also Thomas Cochran, *The Inner Revolution* (New York, 1964), 143.

mental policy itself.²⁰ The antitaxation bias of the community reinforced "the poverty and weakness" of government and vitiated its capacity to withstand bastard pragmatism.

The objective limitations of governmental institutions and their basic incapacity to formulate policy on long-range, scientific considerations made legal process responsive, but not genuinely responsible. Lawmakers reacted to the multitude of special interest demands in the spirit of a consensus favoring productivity: "In no public forum, national or state, was there focused, sustained, public examination of the choices to be made, in degree even minimally measuring up to the stakes. . . . Decision making on matters inherently of public concern [consequently] was atomized into many market decisions. This was done so early and so completely that no public agency ever attempted an overview of the whole."²¹ "The excessive localism and particularity with which legislative process perceived issues delayed the creation of a sensible administrative process capable of coming to terms with the real problems of flexible application of general standards. . . . The legislative process was simply not geared to generalize perception out of particular issues."²²

Men of large vision who held high office apparently made little difference, given the weight of drift and inertia (including laziness, greed, and indifference). There were a few farseeing political leaders whom Hurst can identify as having perceived the larger cost-accounting problems of economic policy. But even they remained "prisoners of the familiar, and of unspoken assumptions" and proved incapable of producing, even as a matter of theory, "a full-blown twentieth century model of legal control."²³

Given the inputs, especially the restlessness, impatience, and materialism of society—and given the inadequacies of governmental capacity—it is not surprising that policy outputs tended to be shaped by "drift and default" instead of by considered, rational decision making. The functions that ought properly to have been exercised by a government of "independent energy" were instead largely assigned to the market.²⁴ The policies adopted were fragmented and were incongruent with the real dimensions of the problems confronting the society. Private energies were mobilized by tort law, the law of contract, and corporation and property law; power was consequently widely dispersed. But in proportion to dispersion of

²⁰ Hurst, *Law and Economic Growth*, 250, 256; see also *ibid.*, 230–31, 246–48. Prior to the Civil War, Hurst has written, a special burden was carried by the lawyer; for in a society that relied upon common law to order its relationships in vital respects, "the lawyer stood out . . . as the guardian and the possessor of a specialized body of knowledge pertaining to man's highest concerns for justice and good order." (Willard Hurst, "The Legal Profession," *Wisconsin Law Review*, 1966 Vol. [Fall 1966], 1969.)

²¹ *Id.*, *Law and Economic Growth*, 123.

²² *Ibid.*, 253.

²³ *Ibid.*, 207; see also *ibid.*, 164–65, and 711, n. 49, and 712–14, nn., on eminent domain and the courts' "disquiet" with regard to legislative mandates.

²⁴ *Ibid.*, 262–63.

power there occurred a loss of coherence and reduction of the visibility of "public interest."²⁵

The key descriptive words, then, in Hurst's portrayal of policy outcomes are drift, default, dispersion, atomization, opportunism, and fragmentation.²⁶ Though government was responsive to particularized and parochial demands, it was effective only in so far as it maintained a politically acceptable—not a rational—distributive policy. Government was ineffective in so far as it failed to bring policy choices to the threshold of perception and rational debate, and as it failed to define policy in each functional area on a basis congruent with the scope of the problem.

The foregoing model of legal process and its effectiveness in the nineteenth century is based, in Hurst's studies, on the empirical evidence of the times. Hurst has greatly enriched our knowledge of that period, for he integrates data from litigation in the trial courts with more traditional evidence drawn from appellate decisions and statutes. He is specifically concerned, moreover, with how law and legal values conditioned the behavior of men in the market place, that is, with the impact of law on informal behavior and institutional functions in the private sector.

Underlying Hurst's historical model is a value-laden theoretical model of how law in a democratic polity ought to function. This theoretical model provides the standard by which Hurst forms his normative judgments both of the American legal process and of policy outcomes. Three distinct normative premises are evident. First, legal process should perform an educative function, sensitizing men to large policy choices so that they may govern themselves rationally.²⁷ Second, legal process ought to be systematic in its fact-finding, scientific in its formulation of general policy from particulars, and thorough in public administration once policies are set. Only so may the constitutional ideal of responsible government be fulfilled, an ideal, Hurst asserts, that coexisted in American thought along with pragmatism and faith in productivity. "The way men do things determines what kind of men they are."²⁸ Loose, irrational procedures for ordering the society will not only reflect but will also perpetuate an irrational legal process.

²⁵ *Ibid.*, 107, 123. On men's "sloth, bafflement, and weariness" as obstacles to rational process, cf. *ibid.*, 140.

²⁶ This is a common characteristic of the studies of Wisconsin's legal history inspired by Hurst. In Friedman, *Contract Law in America*, 147–49, for instance, there is comment on the "waste and futility" of pursuing the public interest through the medium of special charters, on the general lack of "social control," and on social policy as "fragmentary in concept and local in implementation." Similarly, in his analysis of the Wisconsin milldams policy Daniel Dykstra avers that "local economic interests" prevailed and that the legislators lacked "comprehensive understanding . . . of the effect on society of the policies which they were pursuing." (Daniel Dykstra, "Legislation and Change," *Wisconsin Law Review*, 1950 Vol. [Summer 1950], 528, 530.)

²⁷ Hurst, *Law and Social Process*, 164–67, "Themes in United States Legal History," in *Felix Frankfurter: A Tribute*, ed. Wallace Mendelson (New York, 1964), 202–203, 220, and *Law and Economic Growth*, 561, 572.

²⁸ *Id.*, "Themes in United States Legal History," 367.

The third premise of Hurst's normative model is that dispersion of both power and property is a source of individual dignity—indeed, it may provide a vital shield for political rights. Dispersion of this sort is thus entirely consistent with the political and social ideals of a democratic order.²⁹ But ideally legal process must balance dispersion of power against the society's needs over a long-term future. A legal order that performs this balancing function explicitly and rationally will convey its values to the society; it will have an effect on informal behavior and temper bastard pragmatism. One that fails to do so will merely legitimate similar narrowness and default of responsibility in men's private behavior and social relations.³⁰

In sum, then, the ideal of responsible government requires standards of due process in lawmaking. Hurst insists that such due process can be achieved only if government displays independent energy. Otherwise, it cannot act as a force that will give vitality to the conception of "the public interest."³¹ If the legal process fails to meet this standard, according to Hurst's jurisprudential values, the result is no less "magisterial caprice" than, for example, mindless adherence by judges to formal rules and sterile logic. Just as Pound championed "scientific law"—the antithesis of mechanical jurisprudence—so does Hurst hold up this standard of responsible legal process. Hurst finds shortcomings in the quality of the American system's historic performance not because of any mere failure of internal niceties, but rather because procedure can be the stuff of law and because, by pragmatic standards alone, the results of American lawmaking (such as the depletion of the forests) were wasteful.³²

It is important for the historian to inquire whether the American legal process failed to fulfill Hurst's normative criteria because it was simply beyond men's imaginations to conceive of deliberated policy making; or whether, plausibly,

²⁹ *Id.*, *Law and Economic Growth*, 10, 23-34, 56, 106. "The law," Hurst asserts, "contributed to the creation of individuality. Law likewise attested the value we put upon this individuality." (*Id.*, *Law and Social Process*, 114.) "We believed that the unique opportunity in North America was to build a more self-respecting life for the individual out of the expanding options created by an ever mounting curve of material productivity." (*Ibid.*, 121.) The degree to which political rights may be affected by the dispersion of property is a theme pursued in a fresh context by Charles Reich in his studies of "welfare" and "the public-interest state." In contrast to the kind of legal process that Hurst describes for the nineteenth century, the modern administrative apparatus of the twentieth century has tended to downgrade "individual liberty and privacy" in favor of "the interests of the community." (See Charles Reich, "The Law of the Planned Society," *Yale Law Journal*, LXXV [July 1966], 1244, and "The New Property," *ibid.*, LXXIII [Apr. 1964], 771.)

³⁰ Hurst, *Law and Economic Growth*, 26-31, 52-53, 520.

³¹ *Ibid.*, 262. There is an abundant literature on the concept of "the public interest," but for purposes of comparison with Hurst's discussion, see especially Samuel P. Huntington, "Political Development and Political Decay," *World Politics*, XVII (Apr. 1965), 411-12; and William H. Riker, *Democracy in the United States* (2d ed., New York, 1965), 98-104.

³² Roscoe Pound, "Mechanical Jurisprudence," in *Landmarks of Law*, ed. Henson, 101-12. Hurst defines Wisconsin timberlands policy and other nineteenth-century policies for the use of resources as "wasteful" and "irrational" because in the long run they created unnecessary social dislocation. This is a view that embodies the implicit "counterfactual" argument that a different policy—one mandating sustained-yield practices—would have been less costly to the society despite short-run sacrifices of income. (For a discussion of counterfactual argument, see Fritz Redlich, "'New' and Traditional Approaches to Economic History," *Journal of Economic History*, XXV [Dec. 1965], 480-95; see also Harry N. Scheiber, "On the New Economic History," *Agricultural History*, XLI [Oct. 1967], 383-95.)

men may have had understanding of (possibly even actual experience with) "good" legal process, but did not relate it functionally to their resource-allocation problems. Unless I misread Hurst, he believes that the system's failures were in this sense practically inevitable. He says of land policy, for instance, that "some sense of planning responsibility" was present "in the background," but it was not brought to fruition. He asserts, too, that "the legislature, and the country, got about what they actually wanted or were ready to pay for in the way of public lands administration." He explains perpetuation of drift and default by reference to "the environmental odds against major change of direction." Hurst declares elsewhere that even down to the 1880's men failed to adopt a comprehensive policy because they "did not see the long range values."³³

Resolution of this problem depends on whether or not it is indeed plausible to hypothesize that legal process might have worked differently than Hurst indicates it did. If his historical model neglects instances when legal process in the nineteenth century *did* conform closely to the prescriptions of his normative model, then the whole inevitability question has to be reopened.

I would query whether it is correct to argue, as Hurst does, that nineteenth-century Americans were mainly concerned with legal process for material reasons (to sustain rising productivity), albeit tempered by considerations of responsible government. It appears to me debatable, at best, that decision making in noneconomic areas became a focal concern of the people only "grudgingly or as a form of diversion and excitement in spurts of bad conscience over neglected problems."³⁴ This view may underestimate greatly the importance of conflicts over such basic rights as the suffrage—touching the very foundations of law and legal process—which was debated most explicitly in the state constitutional conventions of the 1820's.³⁵ Hurst avers that the nation had finished with "fashioning the principles of power organization" by about 1800. While acknowledging that the constitutional conventions were important episodes, he treats them mainly as part of the larger quest for release of individual energies and for material growth.³⁶ Yet the constitutional conventions embodied "the people's sovereignty; they were unsurpassed arenas of ideological encounter," as Merrill Peterson writes; and they were "potent instruments of reform."³⁷ These debates over "the principles of power organization" reflected cleavages basically different from those Hurst describes in his historical model, for the conventions excited class antagonisms,

³³ Hurst, *Law and Economic Growth*, 22, 60–61, 410.

³⁴ *Id.*, *Law and the Conditions of Freedom*, 29, 43.

³⁵ This subject receives only a few lines' notice, *ibid.*, 30–31.

³⁶ *Ibid.*, 43.

³⁷ *Democracy, Liberty, and Property: The State Constitutional Conventions of the 1820's*, ed. Merrill L. Peterson (Indianapolis, 1966), xv. Popular pressures for reapportionment were often closely linked to economic policy questions; in the South Atlantic States, for example, the underrepresented districts failed to obtain their full share of state expenditures for internal improvements; this impelled them to seek constitutional reform. (Fletcher M. Green, *Constitutional Development in the South Atlantic States, 1776–1860* [Chapel Hill, N. C., 1930; reprinted New York, 1966], 150–52, 161–63.)

basic sectional divisions, and, not least important, conflicting ideologies. It is impossible, I think, to find only a single *Volksgeist* or "consensus" when one examines these conflicts. Decisions on such issues as the suffrage or representational apportionment could not be disaggregated and dispensed piecemeal, as happened with resource-allocative policy (water power, timber resources, minerals, and the like). Demands were well focused, and large policy was in the forefront; political status and rights could not be doled out on an *ad hoc* basis to atomistic, particularized interests.

If this argument is correct, it raises a second basic question of historical interpretation: does Hurst's model accurately (indeed brilliantly) describe one, *but only one*, type of historic American legal process?³⁸ This question is pertinent, I think, even if one narrows the scope of inquiry to embrace only government and the economy. Consider especially the legal process as it worked in the states that undertook major internal improvements in the early canal era. The main task of the policy process then was governmental construction and operation of bulk transport. Wisconsin's initial state constitution, to be sure, prohibited such activity by the state government, and it barred incurring of debt for internal improvements.³⁹ As a result, the main focus of economic policy in the Wisconsin case was on the state government's allocation of seemingly abundant resources and on the ordering of the market through contract, tort, and property law. But the central focus of policy rested elsewhere in New York, Ohio, Pennsylvania, Michigan, Indiana, and numerous other states. In fact, their ambitious efforts (and accompanying fiscal disasters) in canal projects and other transport enterprises impelled Wisconsin to eschew such activity.⁴⁰

In the early period of canal construction in these states, only public enterprise appeared feasible. To undertake state enterprise, in turn, required mobilizing capital, that is, mobilizing a scarce resource. The success of each enterprise depended on erecting a bureaucratic structure that far exceeded in size and complexity any official agency of the state of Wisconsin in the mid-nineteenth century. The products of policy making, moreover—the canals and other bulk transport

³⁸ The following critique of Hurst's policy process model owes much to the theory of "arenas of policy" presented by Theodore J. Lowi, in "American Business, Public Policy, Case-Studies, and Political Theory," *World Politics*, XVI (July 1964), 677-715. One type of policy in Lowi's scheme conforms precisely to Hurst's portrayal of the Wisconsin lumber policy: it is the arena of "distributive" policy, which embraces resource allocation and the devolution of governmental largesse. (Consistent with Hurst's view, Lowi finds that distribution was "almost the exclusive type of national policy from 1789 until virtually 1890.") Because resources distributed by government can be "disaggregated and dispensed unit by small unit," distributive policies tend to be "not policies at all, but . . . highly individualized decisions. . . ." (*Ibid.*, 689-90.) Lowi distinguishes the distributive arena from the "regulatory" arena, in which policy cannot be disaggregated so thoroughly, so that manifest deprivation and denial must occur. Third is the "redistributive" arena, in which disaggregation is nearly impossible since "the categories of impact are much broader, approaching social classes"; examples of policy in this arena are income taxation and broad welfare-state measures such as Social Security. (*Ibid.*, 691, 703-704.)

³⁹ See Frank Mallare, "Comment: Wisconsin's Internal Improvements Prohibition," *Wisconsin Law Review*, 1961 Vol. (Mar. 1961), 294.

⁴⁰ *Ibid.*, 296-98. The experience of the canal states is examined in Goodrich, *Government Promotion*, *passim*.

facilities—would at first benefit only a few regions and deprive other localities of equal advantages.⁴¹ Finally, the social costs were to be felt immediately, in the burden of taxation to service and amortize state debts. Though men commonly underestimated the debt to be incurred, or overestimated the canals' revenue potential, there was no escaping the tax question on grounds that it was a busy society, not much interested in taxation as part of larger developmental policy.

The resultant variations from Hurst's historical model of "process" were numerous and dramatic. First, a ranking of priorities among contending interests could not be evaded, as certain regions would be indulged and others deprived. Second, the engineers characteristically became spokesmen for long-range planning and comprehensive principles for development of the transport system. Nineteenth-century engineers dedicated much effort to rational calculation of costs and benefits, even to quantification, much as engineers do today. From the regional and functional interest groups, in turn, came pressure of a more particularized kind, admittedly much like the pressures exerted by atomized interests in the Wisconsin lumber industry. But there was another dimension even to these pressures: men articulated their demands for transport not only by a calculus of productivity, but also in terms of equity. Egalitarian precepts required, ideally, that burdens (taxes) and benefits (transport facilities) be distributed equally among all districts of a state. Though material self-interest was hardly lacking, the egalitarian ideal conditioned policy processes significantly. On the basis of men's actions as well as their rhetoric, one can, I think, make as good a case for the impact of egalitarian ideals as one can make for Hurst's version of the contemporary *Volksgeist*, which stresses the primacy of more materialistic social goals.⁴² Rather than a consensus of popular views, there was a tension among three distinct and competing "validating principles": planning, particularism, and ideology qua egalitarianism.⁴³ Moreover, in the policy debates that surrounded the granting of special charters to railroads, when the private sector finally acquired the fiscal capacity to build bulk transport lines, the public canal officials did operate with independent energy. Although in the end they were overwhelmed, they did bring general policy issues past the threshold of consciousness, and they forced railroad promoters to adduce cost-benefit analyses of their own to justify

⁴¹ Of course, once the fiscal restraints on expansion of state canal systems disappeared in the mid-1830's, the by-passed (deprived) regions of each state successfully pressed for the construction of new ancillary lines and "comprehensive programs." (See Harvey H. Segal on "the role of rivalry," in *Canals and American Economic Development*, ed. Carter Goodrich [New York, 1963], 176-79; Harry N. Scheiber, "Urban Rivalry and Internal Improvements in the Old Northwest, 1820-1860," *Ohio History*, LXXI [Oct. 1962], 232-35; Ronald Shaw, *Erie Water West: A History of the Erie Canal, 1792-1854* [Lexington, Ky., 1966], 305-13.) On the size and tasks of state bureaucracy where canals were built as public enterprises, and the "independent energy" that such public agencies displayed, see Nathan Miller, *The Enterprise of a Free People: Aspects of Economic Development in New York State during the Canal Period, 1792-1838* (Ithaca, N. Y., 1962); and Harry N. Scheiber, *Ohio Canal Era: A Case Study of Government and the Economy, 1820-1861* (Athens, Ohio, 1969), Chaps. I, IV, XIII.

⁴² See Hurst, *Law and Economic Growth*, 28, 572, on action as an expression of thought in "this unphilosophical society."

⁴³ Scheiber, *Ohio Canal Era*, 88-92, 355-58.

charters and other public aid (including state and local subsidies) for their projects.⁴⁴

Further historical inquiry may suggest the need to revise Hurst's model further, to allow for still a third major arena of economic policy which was distinct from either resource allocation or transport building. This third arena was economic regulation, especially as it concerned banks and corporations. Hurst has perhaps given insufficient weight to the impact of ideological and partisan cleavages on such issues. In essence, Hurst takes the victor's view of history: he emphasizes that Democratic legislatures churned out special charters despite Jacksonian strictures against such "monopolies" and that "eventually" legislatures ceased to "deal by mere fiat" with the banking mechanism.⁴⁵ But this misses the point that in certain states, for periods of varying duration, the very question of whether capitalism was to be permitted to spawn larger-scale forms of organization became an intensely felt issue in politics, occasionally even delaying the course of capitalist development on new lines.⁴⁶ To relegate these controversies and struggle to the category of mere interim is to risk hiding from sight a segment of policy process that is basically distinguishable from the Hurst model.

My final caveat relates to the very concept of *Volksgeist* and consensus, which lies at the heart of Hurst's analyses.⁴⁷ In the South was a segment of society where deference politics were perpetuated, where maintenance of a caste system (militating against status mobility) was of high priority, and where the expectant capitalist apparently encountered deeply rooted prejudices favoring continued agrarian dominance of the society.⁴⁸ To say that even in the North and West we were all Federalists, all Republicans "in possessing a common instrumental belief which shaped the nineteenth century legal order," may treat too lightly the political conflicts that once loomed so large in historical analysis.⁴⁹ The Democrats

⁴⁴ *Ibid.*, Chap. x; Frederick Merk, "Eastern Antecedents of the Grangers," *Agricultural History*, XXIII (Jan. 1949), 1-8. Although the administrators of state and federal land policy did not exhibit the same degree of initiative, there was abundant attention to ideological questions in the formulation of public policy in this area. (See Mary E. Young, "Congress Looks West: Liberal Ideology and Public Land Policy in the Nineteenth Century," in *The Frontier in American Development: Essays in Honor of Paul Wallace Gates* [Ithaca, N. Y., 1969], 74-112.)

⁴⁵ Hurst, *Law and the Conditions of Freedom*, 56, 53-54, *et passim*.

⁴⁶ A case in point is the banking policies of Ohio, where the pendulum of regulation swung from one limit to the other as Whig and Democratic majorities alternated in successive legislatures in the 1830's and early 1840's. (See Francis P. Weisenburger, *The Passing of the Frontier* [Columbus, Ohio, 1941], 337-62; Scheiber, *Ohio Canal Era*, 145-54; cf. Michael A. Lebowitz, "The Jacksonians: Paradox Lost?" in *Towards a New Past: Dissenting Essays in American History*, ed. Barton Bernstein [New York, 1968], 65-89.)

⁴⁷ Hurst, *Law and Social Process*, 254-55, and *Law and the Conditions of Freedom*, 6-10, 33.

⁴⁸ Green, *Constitutional Development*, 162, 170, and Chap. xiv; Eugene Genovese, *The Political Economy of Slavery* (New York, 1965), 180-220; Stanley M. Elkins and Eric McKittrick, "A Meaning for Turner's Frontier," *Political Science Quarterly*, LXIX (Sept., Dec. 1954), 321-53, 565-602, in which a restatement is attempted of the fundamentally different political styles of the West and Southwest.

⁴⁹ Hurst, *Law and the Conditions of Freedom*, 33. Cf. the discussion in Seymour Martin Lipset, *The First New Nation: The United States in Historical and Comparative Perspective* (New York, 1963; reprint ed., Garden City, N. Y., 1967), 54-68. The concept of a historic American "consensus," popular among political scientists, requires re-evaluation in light of Hurst's evidence respecting dynamic change within the federal system, as the result of independent legal processes in the individual states,

may have been Whigs in disguise, but, if so, they maintained their false identity for a very long period, especially considering the depth and intensity of party differences—in the states, on which Hurst has so brilliantly focused attention—on such issues as the privileges to be granted corporations, or the regulation of banking.

These are difficult questions. The full depth of their perplexities and their importance have become clear only because Hurst's confrontation of legal-economic history has been so productive. He has built on a confidence that the concrete particulars of life are worthy of attention from mature scholars in law and history. He has demonstrated how the legal process defined the market and how it set rules for operation of the price system (mechanisms that often are treated as disembodied abstractions by even eminent economic historians). Moreover, Hurst has portrayed with rare acuity the complex interplay of our legal system with cultural traits and values, thereby providing a conceptual basis for fresh re-evaluation of the core issues in jurisprudence—all this mainly with the stuff of local history, a perilous field (because it can be so workaday) even for scholars with far less ambitious research purposes. It is thus appropriate to close with Hurst's own observation, in another context, that "as men expanded their knowledge, they enlarged their ignorance too—specialization drove creative minds farther apart rather than closer together."⁵⁰ In an era of intensive academic specialization, Hurst's studies are valuable, above all, because they remind us that the historian must deal with the whole fabric of life. He has made this great borderland between law and history a far richer and more challenging place than when he first began to explore it.

and dynamic change in the content of state policies. (See Samuel P. Huntington, "Political Modernization: America vs. Europe," *World Politics*, XVIII [Apr. 1966], 406–407 *et passim*; cf. Harry N. Scheiber, *The Condition of American Federalism: An Historian's View* [89 Cong., 2 sess., Senate Committee on Government Operations, print, Oct. 15, 1966], 1–12; and Carl J. Friedrich, *Trends of Federalism in Theory and Practice* [New York, 1968], 7–9.)

⁵⁰ Hurst, *Law and Social Process*, 70.

The Creation of Nobles in Prussia, 1871–1918

LAMAR CECIL

THE attention directed by contemporary historians to the German Empire founded by Otto von Bismarck and William I has tended to focus either on men and movements opposed to the established authorities and entrenched interests that prevailed in the kingdom of Prussia or on the principal military, diplomatic, and agrarian figures whose frequently conflicting aspirations the monarchy was pressed to resolve. Because of the provisions of the imperial constitution of 1871, those who ruled in Prussia also wielded a commanding influence in imperial affairs.

Sympathetic analyses have revealed the zeal but eventual frustration of bourgeois political parties and proletarian movements that challenged both state and society in pre-1914 Germany.¹ Historians have also dwelt on the intellectual malaise encountered at the end of the century in men such as Houston Stewart Chamberlain, Paul de Lagarde, and Adolf Stöcker, all of whom were vociferous critics not only of non-Aryans but of what they diagnosed as Germany's Philistine character and deficient leadership.² At the same time, historians have subjected the military and diplomatic stalwarts of William I and his successors to the most exacting scrutiny, and some investigation has been begun into the agricultural

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¹ See in particular the many distinguished works published under the auspices of the *Kommission für Geschichte des Parlamentarismus und der politischen Parteien*, edited by Werner Conze et al.: James J. Sheehan, *The Career of Lujo Brentano: A Study of Liberalism and Social Reform in Imperial Germany* (Chicago, 1966); Carl E. Schorske, *German Social Democracy, 1905–1917: The Development of the Great Schism* (Cambridge, Mass., 1955); Vernon L. Lidtke, *The Outlawed Party: Social Democracy in Germany, 1871–1890* (Princeton, N. J., 1966); Peter Gilg, *Die Erneuerung des demokratischen Denkens in Wilhelminischen Deutschland: Eine Ideengeschichtliche Studie zur Wende vom 19. zum 20. Jahrhundert* (Wiesbaden, 1965); Peter Molt, *Der Reichstag von der improvisierten Revolution* (Cologne, 1963); Thomas Nipperdey, *Die Organisation der deutschen Parteien vor 1918* (Düsseldorf, 1961); Klaus Epstein, *Matthias Erzberger and the Dilemma of German Democracy* (Princeton, N. J., 1959), as well as Jürgen Kuczynski, *Die Geschichte der Lage der Arbeiter unter dem Kapitalismus*, III–V, XII–XIV, XVIII–XX (38 vols., Berlin, 1961–69).

² E.g., Robert W. Lougee, *Paul de Lagarde, 1827–1891: A Study of Radical Conservatism in Germany* (Cambridge, Mass., 1962); Fritz Stern, *The Politics of Cultural Despair: A Study in the Rise of the Germanic Ideology* (Berkeley, Calif., 1959); and George L. Mosse, *The Crisis of German Ideology: Intellectual Origins of the Third Reich* (New York, 1964). Harry Young, *Maximilian Harden, Censor Germaniae: The Critic in Opposition from Bismarck to the Rise of Nazism* (The Hague, 1959); Wolfgang J. Mommsen, *Max Weber und die deutsche Politik, 1890–1920* (Tübingen, 1959); Utz-Friedebert Taube, *Ludwig Quide: Ein Beitrag zur Geschichte des demokratischen Gedankens in Deutschland* (Kallmünz, 1963); and Joachim Remak, *The Gentle Critic: Theodor Fontane and German Politics, 1848–1898* (Syracuse, N. Y., 1964), are of interest for other sorts of critics.

society of the east Elbian regions of Prussia.³ But many of the exalted pillars as well as the glamorous appendages of imperial power, especially those uninvolved with political office, have been neglected. The Prussian court and its courtiers, the nobility, the *salonières* of Berlin society, the bourgeois plutocracy, the aristocratic cliques of Silesia, East Prussia, and the Rhineland continue to be shadow figures, of recognized importance but of undefined character.⁴ Even the crowned heads of Prussia and the Empire have long been the property of amateur rather than professional historians.⁵

³ On the military, see Gordon A. Craig, *The Politics of the Prussian Army, 1640-1645* (New York, 1956); Gerhard Ritter, *Staatskunst und Kriegshandwerk: Das Problem des "Militarismus" in Deutschland* (4 vols., Munich, 1954-68); Karl Demeter, *Das deutsche Offizierkorps in seinen historisch-soziologischen Grundlagen* (Berlin, 1930); Walther Hubatsch, *Die Ära Tirpitz: Studien zur deutschen Marinepolitik, 1890-1918* (Göttingen, 1955), and *Der Admiralstab und die obersten Marinebehörden in Deutschland, 1848-1945* (Frankfurt a.M., 1958); Martin Kitchen, *The German Officer Corps, 1890-1914* (New York, 1969); and Jonathan Steinberg, *Yesterday's Deterrent: Tirpitz and the Birth of the German Battle Fleet* (London, 1965). Much of the best literature on diplomacy has centered around the Bismarcks and Friedrich von Holstein. Otto Pflanze, *Bismarck and the Development of Germany* (1 vol. to date, Princeton, N. J., 1963-); biographical introd. by Walter Bussmann in his *Staatssekretär Graf Herbert von Bismarck: Aus seiner politischen Privatkorrespondenz* (Göttingen, 1964); *The Holstein Papers: The Memoirs, Diaries and Correspondence of Friedrich von Holstein, 1837-1909*, ed. Norman Rich and M. H. Fisher (4 vols., Cambridge, Eng., 1955-63); Norman Rich, *Friedrich von Holstein: Politics and Diplomacy in the Era of Bismarck and Wilhelm II* (2 vols., Cambridge, Eng., 1965); Helmuth Rogge, *Holstein und Hohenlohe: Neue Beiträge zu Friedrich von Holsteins Tätigkeit als Mitarbeiter Bismarcks und als Ratgeber Hohenlohes . . .* (Stuttgart, 1957), and *Holstein und Harden: Politisch-publizistisches Zusammenspiel zweier Aussenseiter des Wilhelminischen Reichs* (Munich, 1959); Günther Richter, *Friedrich von Holstein: Ein Mitarbeiter Bismarcks* (Lübeck, 1966). For agriculture, see Hans Rosenberg, *Grosse Depression und Bismarckzeit: Wirtschaftsablauf, Gesellschaft und Politik im Mitteleuropa* (Berlin, 1967), and "Die Demokratisierung der Rittergutsbesitzerklasse," *Zur Geschichte und Problematik der Demokratie: Festgabe für Hans Herzfeld*, ed. Wilhelm Berges and Carl Hinrichs (Berlin, 1958), 459-86; Hans-Jürgen Puhle, *Agrarische Interessenpolitik und preussischer Konservatismus im Wilhelminischen Reich, 1883-1914* (Hanover, 1967); Robert E. Dickinson, *Germany: A General and Regional Geography* (New York, 1953); Sarah R. Tirrell, *German Agrarian Politics after Bismarck's Fall: The Formation of the Farmers' League* (New York, 1951); Alexander Gershenkron, *Bread and Democracy in Germany* (Berkeley, Calif., 1943); and Richard W. Tims, *Germanizing Prussian Poland: The H-K-T Society and the Struggle for the Eastern Marches in the German Empire, 1894-1919* (New York, 1941).

⁴ The neglect, however, has not been total. See J. C. G. Röhl, *Germany without Bismarck: The Crisis of Government in the Second Reich, 1890-1900* (Berkeley, Calif., 1967); Hartmut Kaelble, *Industrielle Interessenpolitik in der Wilhelminischen Gesellschaft: Centralverband Deutscher Industrieller, 1895-1914* (Berlin, 1967); Lamar Cecil, *Albert Ballin: Business and Politics in Imperial Germany, 1888-1918* (Princeton, N. J., 1967); Hans Jaeger, *Unternehmer in der deutschen Politik (1890-1918)* (Bonn, 1967); Walter Görlitz, *Die Junker: Adel und Bauer im deutschen Osten, Geschichtliche Bilanz von 7 Jahrhunderten* (3d rev. ed., Limburg, 1964); Heinz Gollwitzer, *Die Standesherren: Die politische und gesellschaftliche Stellung der Mediatisierten, 1815-1918* (2d rev. ed., Göttingen, 1964); Nikolaus von Preradovich, *Die Führungsschichten in Österreich und Preussen (1804-1918): Mit einem Ausblick bis zum Jahre 1945* (Wiesbaden, 1955); Rudolf Morsey, *Die oberste Reichsverwaltung unter Bismarck, 1867-1890* (Münster, 1957); Lysbeth Walker Muncy, *The Junker in the Prussian Administration under William II, 1888-1914* (Providence, R. I., 1944); Otto Count zu Stolberg-Wernigerode, *Die unentschiedene Generation: Deutschlands konservative Führungsschichten am Vorabend des Ersten Weltkrieges* (Munich, 1968); Elisabeth Fehrenbach, *Wandlungen des deutschen Kaisergedankens, 1871-1918* (Munich, 1969); Ernst Kohn-Bramstedt, *Aristocracy and the Middle Classes in Germany: Social Types in German Literature, 1830-1900* (London, 1937); and, in a more popular vein, Erich Achterberg, *Berliner Hochfinanz: Kaiser, Fürsten, und Millionäre um 1900* (Frankfurt a.M., 1965).

⁵ William II has been better served than Frederick III, Frederick better than William I, for whom one must still rely on Erich Marcks's *Kaiser Wilhelm I* (8th rev. ed., Munich, 1918). First published in 1897, it has since gone through many revised editions. The only recent studies on Frederick III are Werner Richter, *Kaiser Friedrich III* (Zurich, 1938); and Andreas Dorpalen, "Emperor Frederick III and the German Liberal Movement," *American Historical Review*, LIV (Oct. 1948), 1-31. There is, however, much information on the unhappy man in various biographies of his wife, especially Egon Conte Corti, *Wenn . . . : Sendung und Schicksal einer Kaiserin* (Graz, etc., 1954), translated by E. M. Hodgson as *The English Empress* (London, 1957). The bibliography on William II is vast but ama-

The historical exhumation made here of the men raised into or promoted within the Prussian nobility between 1871 and 1918 is in the first place intended to extend, very modestly, our knowledge of the three Kings responsible for these creations. Additions to, or promotions within, the Prussian nobility in the late nineteenth and early twentieth centuries were entirely under the jurisdiction of the crown. Ministers or courtiers might suggest candidates for honors, but the sovereign's reaction to such proposals was decisive and final. Often the monarch bestowed titles without consulting his advisers. In the second place, a study of the nobility should illuminate its institutional character by analyzing the composite character of its membership, differences between nobles of various ranks, and the reasons that led to the creation of new nobles or the promotion of old ones. Finally, it should advance our understanding of landholding as well as bureaucratic and military service, activities in which Prussia's nobles, new and old, were much involved.

There are, fortunately, two catalogues of titles granted by the Kings of Prussia between 1871 and 1918: Adolf Maximilian Gritzner, *Chronologische Matrikel der brandenburgisch-preussischen Standeserhöhungen und Gnadenakte* (Berlin, 1874), and Albrecht Freiherr von Houwald, *Brandenburg-preussische Standeserhöhungen und Gnadenakte* (Görlitz, 1939).⁶ These two sources provide merely the name of the newly created or advanced noble, the date of creation or advance, occasionally some reference to the reason for the award, and, in the case of Houwald, a description of the armorial bearings granted with the patent.⁷ The tabulations below deal with a total of 1,315 creations or promotions, 1,094 to the

teur; many of the works are recent. None are imaginative or authoritative, but the best are Erich Eyck, *Das persönliche Regiment Wilhelms II: Politische Geschichte des deutschen Kaiserreiches von 1890 bis 1914* (Erlenbach-Zürich, 1948); and Michael Balfour, *The Kaiser and His Times* (London, 1965).

⁶ Gritzner covers the period 1600-1873; Houwald, 1873-1918. Gritzner seems to be complete, and I have been able to find only three creations that escaped Houwald.

⁷ *Standeserhöhungen* were elevations from bourgeois status to that of knight (*von*), baron (*Freiherr*), count (*Graf*), prince (*Fürst*), or duke (*Herzog*), or promotions within the noble ranks. *Gnadenakte* apply to recognitions or confirmations of obscured claims to titles already awarded, to changes of name or arms, and to certain dynastic titles. Not all *Standeserhöhungen* and *Gnadenakte* are included in the calculations made here. Female creations of all ages and minor males have been excluded since their elevations did not proceed from their own merits but from those of their parents or other relatives. Forty-one women and thirteen minor males were thus omitted. Also not included are elevations of whole branches of families not mentioned by individual patent but by a general grant covering anyone successfully advancing a claim of descent through this branch. These I have excluded: first, because it is difficult to determine with precision how many people were affected; second, because the elevations made in this way were due not to the accomplishments of the new nobles but rather to some claim to nobility, or some service performed, by an ancestor. Between 1871 and 1918 there were 27 such wholesale creations, resulting in the ennobling of perhaps 250 persons. Note that when large numbers of men from the same family were simultaneously created but were *individually* specified in the collective patent they are entered in the calculations. Here there was a distinct intention on the part of the monarch to elevate these members of the family, but not others. Another form of *Standeserhöhung* eliminated from consideration here was an award resulting from dynastic complications in royal or mediatised houses. A Prussian prince of the blood, for example, on renunciation of his succession or property rights following an *unebenbürtig* marriage, might receive a lower, nonroyal title of count or the like. There were fourteen such creations during the period of the Empire. Finally, *Gnadenakte* resulting only in alterations of name or arms have been disregarded.

rank of *von*, 151 to baron, 54 to count, 15 to prince, and one to duke.⁸ Of these 1,315 men, 1,129 were born bourgeois while 186 were already Prussian nobles and were now raised to a higher rank in the nobility. William I was responsible for 419 new creations or advances, Frederick III for 60,⁹ and William II for 836.

A variety of sources, but especially the aristocratic annual known commonly as the *Gotha*, can be employed to form a portrait of most of the new or newly advanced nobles.¹⁰ It is not always possible to obtain complete biographical data, however, especially with regard to parentage. And in the few cases in which the noble was never listed in the *Gotha* or elsewhere, the skeletal reference in Gritznor or Houwald is the only information available. In this study, a code was assigned to each category of information (parentage, occupation, and so forth), and this material was then processed by an IBM 7094 computer.¹¹

Advances within, rather than additions to, the nobility were more common under the first two Kings than under the last, and therefore those receiving favors from William I and his son had more aristocratic sires than did those who were beholden to William II (see Tables I and II, pp. 761, 762-63). In advancing nobles, William I was more likely than his successors to choose men who had aristocratic mothers as well as fathers. William I's preference for a noble maternal connection is manifest in those men whom he promoted from bourgeois to noble status. Almost 28 per cent of such elevations had noble mothers, a percentage considerably higher than that of similar creations made by either his son or grandson. Frederick III's taste for promoting old nobles along with creating new ones was even more pronounced than that of his father (see Table I, p. 761). Those whom he advanced were, however, twice as likely as William I's promotions to have bourgeois

⁸ Men who were elevated more than once (nineteen cases) are tabulated only one time, in the highest rank awarded.

⁹ Because the sample is small, caution must be observed in rendering the figures for Frederick III's creations into percentages. The same reservation applies to other samples of limited size, such as dukes, princes, counts, and so forth.

¹⁰ The *Gotha*, first published at Gotha in 1764, appeared under various titles until 1942. Under the Empire, the title was usually the *Gothaisches Genealogisches Taschenbuch*, and it was issued in separate volumes for each rank except duke, with the *von*'s subdivided according to date of title (e.g., *Gothaisches Genealogisches Taschenbuch der Uradeligen/Briefadeligen/Freiherrlichen/Gräflichen/Fürstlichen Häuser*). The pursuit of nobles in the pages of the *Gotha* is very taxing. A noble's name is usually, but not always, entered in the edition appearing shortly after his creation, but subsequent editions are likely to carry biographical information not given in the original entry. A continuation of the *Gotha*, *Genealogisches Handbuch des deutschen Adels* (44 vols. to date, Glücksburg, 1951-), also appears in separate volumes for each rank and subdivided for all ranks according to date of creation. This source is considerably less valuable, however, since its genealogies concentrate on living members of the families rather than on their nineteenth-century ancestors. The *Handbuch des preussischen Adels* (2 vols., Berlin, 1892) is also of value. Where all of these failed, the missing information could sometimes be culled from the following: *Wer Ist's* (10 vols., Berlin, 1905-35); *Allgemeine Deutsche Biographie* (56 vols., Leipzig, 1875-1912); *Neue Deutsche Biographie* (8 vols. to date, Berlin, 1952-); *Biographisches Jahrbuch und deutscher Nekrolog* (18 vols., Vienna, 1896-1917); *Deutsches Biographisches Jahrbuch* (11 vols., Berlin, 1925-32); and other standard biographical manuals.

¹¹ Mmes. Andrea Smith and Shirley Gilbert of the Office for Statistical Study and Survey Research at Princeton University kindly introduced me to the complexities of data processing. Professor John R. Gillis provided a number of bibliographical suggestions. The staff of the Genealogical Division of the New York Public Library greatly facilitated my research.

mothers, although they had been more successful in finding noble wives by the time of their promotion, or shortly thereafter. William II was much more inclined to raise men from bourgeois to noble status than to promote old aristocrats to more exalted ranks; thus fewer of his creations could claim noble fathers, though many of them had aristocratic mothers or wives, and in some cases both. In all

Table I
Creations of Various Ranks by Various Monarchs (All Creations)¹²

	William I		Frederick III		William II	
	No.	Per cent	No.	Per cent	No.	Per cent
Duke	—	—	—	—	1	.12
Prince	1	.24	2	3.33	12	1.44
Count	16	3.82	7	11.67	31	3.71
Baron	89	21.24	9	15.00	53	6.34
<i>von</i>	313	74.70	42	70.00	739	88.39
Total	419	100.00	60	100.00	836	100.00

three reigns, about one creation in three went to men who had brothers or other close relatives similarly favored by one of the three sovereigns (see Table II, pp. 762-63).

Agricultural pursuits were the most frequent occupation of the new nobles' fathers, varying from 33 per cent under Frederick III to 25 per cent under William II (see Table III, pp. 765-66). About 14 per cent of the favors awarded by each of the three monarchs went to sons of fathers who were in military service. Of these

¹² Hereafter the following abbreviations will be used in the tables:

W₁ = William I
F₃ = Frederick III
W₂ = William II

v = *von*
B = baron
C = count
P = prince
D = duke

Prot = Protestant
RC = Roman Catholic
geh = geheim
Reg = Regierung
unk = unknown
FO = Foreign Office
ha. = hectares

Pr = Prussia
xPr = outside Prussia
PrxPr = in and outside Prussia
imp = imperial

Fkm = *Fideikommiss*
Bbg = Brandenburg
EP = East Prussia
Hano = Hanover
Hess = Hesse
Pom = Pomerania
Pos = Posen

Rhld = Rhineland
Sax = Saxony
SH = Schleswig-Holstein
Sil = Silesia
Wfal = Westphalia
WP = West Prussia

Information in parentheses following the description of each table indicates the sample used to determine the percentages given.

Table II
Mothers, Fathers, and Wives of Noble Birth, Blood Relationship to Other Creations, Age (All Creations for Whom Relevant Biographical Information Is Complete)

William I (419 Creations)	Frederick III (60 Creations)	William II (836 Creations)
Percentage: information complete for 379 cases or 90.45% of all creations Noble mother Noble father Both parents noble One parent noble Neither parent noble Total	Percentage: information complete for 56 cases or 93.33% of all creations Noble mother Noble father Both parents noble One parent noble Neither parent noble Total	Percentage: information complete for 776 cases or 92.82% of all creations Noble mother Noble father Both parents noble One parent noble Neither parent noble Total
No. 105 94 66 67 246 379	No. 7 13 5 10 41 56	No. 138 79 48 121 607 776
Per cent 27.70 24.80 17.41 17.60 64.99 100.00	Per cent 12.50 23.21 8.92 17.84 73.24 100.00	Per cent 17.78 10.18 6.19 15.59 78.22 100.00
Marriage: information complete for 394 cases or 94.03% of all creations Married to noble wives before or within 6 months of creation	Marriage: information complete for 59 cases or 98.33% of all creations Married to noble wives before or within 6 months of creation	Marriage: information complete for 772 cases or 92.34% of all creations Married to noble wives before or within 6 months of creation
115 29.19	23 38.98	219 28.37

Table II (continued)

William I (419 Creations)	Frederick III (66 Creations)	William II (836 Creations)
Noble relatives: information complete for all cases	Noble relatives: information complete for all cases	Noble relatives: information complete for all cases
brothers, stepbrothers, fathers, stepfathers, sons, uncles, or nephews also created, 1871-1918	brothers, stepbrothers, fathers, stepfathers, sons, uncles, or nephews also created, 1871-1918	brothers, stepbrothers, fathers, stepfathers, sons, uncles, or nephews also created, 1871-1918
144 34.39	17 28.33	274 32.78
Number reporting one or more noble relationships listed above	Number reporting one or more noble relationships listed above	Number reporting one or more noble relationships listed above
268 63.96	36 60.00	488 58.37
Average age	Average age	Average age
48.04	57.24	54.77

fathers, almost 80 per cent served in the Prussian Army, for the most part attaining some rank between captain and colonel. Slightly under 4 per cent were in the imperial navy, and the balance served in the armies of other German states, in which a large proportion (40 per cent) achieved general rank. Approximately one elevation in every six went to a man whose father was employed in the bureaucracy, with almost 80 per cent of these fathers holding positions in the Prussian service and almost all of the rest in the administrations of various of the other German states rather than in the imperial bureaucracy.¹³ Like the fathers in the military, those who were Prussian civil servants were usually in the middle ranks and were concentrated in some branch of the judiciary. Frederick III was conspicuously less likely to grant titles to sons of bureaucratic fathers, being much more partial than his father and rather more than his son to men whose fathers were active in business or banking (see Table III, pp. 765-66).

An overwhelming majority of the new, or newly advanced, nobles themselves—steady at about 85 per cent throughout the Empire—were Protestant (see Table IV, p. 768), a percentage considerably higher than the proportion of Protestants in the Prussian population just before the First World War (62 per cent) (see Table IV, p. 768). Frederick III awarded titles to only two Jews, but this number raises the percentage of Jews among his creations to a level considerably higher than that attained under either William I or William II.¹⁴ In selecting new peers or in advancing old ones, William I was partial to men of military background, inclining considerably more than his son, and rather more than his grandson, to bring forward not only men who were professional soldiers but those in other occupations who earlier had served in the armed forces (see Tables IV and V, pp. 768, 770).¹⁵ William I was very partial to officers in the ranks of colonel or below, while his son showed a preference for awarding titles to generals. William II tended to divide his honors between the two groups (see Table VI, p. 772). The nobles created by both Williams were less likely to be landlords, either as professionals or as incidental holders, than those raised by Frederick III (see

¹³ In the early years of the Empire few fathers of new nobles would have served in the imperial navy or bureaucracy since in most cases the fathers' careers ended before 1871. It is curious, however, that the number of fathers in imperial service does not increase significantly in the later years of the Empire.

¹⁴ In 1910 Jews numbered just over 1 per cent of the Prussian population, a figure that varied only slightly during the Empire. (See Prussia, Statistisches Amt, *Statistisches Jahrbuch für den preussischen Staat*, 1914 [Berlin, 1915], 15.) Since the *Gotha* lists baptized Jews as Protestant or Catholic it is difficult to determine accurately how many men who were born Jewish were ennobled. For criticism of Frederick III's grants of the *von* to Minister of Justice Heinrich Friedberg and to *Reichsgerichtspräsident* Eduard Simson, see Ladislaus Count Szögyényi-Marich (Austro-Hungarian ambassador in Berlin) to Foreign Minister Gustav Count Kálnoky, Mar. 17, 24, 1888, in Preussen III, CXXXIII (*Berichte*), Haus- Hof- und Staatsarchiv, Vienna [hereafter cited as HHStA]; and *Denkwürdigkeiten des General-Feldmarschalls Alfred Grafen von Waldersee*, ed. Heinrich O. Meisner (3 vols., Stuttgart, 1923-25), I, 376, 394, 402.

¹⁵ In a few cases doubt arose as to how a noble was to be counted with regard to occupation. The rule applied was to list him in that occupation which consumed most of his time but which did not necessarily produce most of his income. Thus, for example, a fifty-five-year-old army captain also a landlord is presumed to have been in the inactive reserves and is classified as a landlord. A twenty-five-year-old lieutenant, also a landowner, known to have continued later in life in army service, is listed as an army officer.

Table III
Father's Occupation (All Creations)

Occupation	W ₁ No.	W ₁ Per cent	F ₃ No.	F ₃ Per cent	W ₂ No.	W ₂ Per cent	D No.	D Per cent	P No.	P Per cent	C No.	C Per cent	B No.	B Per cent	v No.	v Per cent
Agriculture Pr	112	26.73	19	31.67	203	24.22	1	100.00	12	80.00	29	53.75	72	47.67	220	20.11
Agriculture xPr	4	.95	1	1.67	6	.72	—	—	—	—	2	3.70	4	2.65	5	.46
	116	27.68	20	33.34	209	24.94	1	100.00	12	80.00	31	57.45	76	50.32	225	20.57
Army Pr	56	13.37	8	13.33	85	10.17	—	—	1	6.67	6	11.10	22	14.57	120	10.98
Army xPr	20	4.77	—	—	12	1.44	—	—	—	—	—	—	8	5.30	24	2.20
Navy	1	.24	—	—	6	.72	—	—	—	—	—	—	1	.66	6	.55
	77	18.38	8	13.33	103	12.33	—	—	1	6.67	6	11.10	31	20.53	150	13.73
Bureaucrat Pr	65	15.51	3	5.00	114	13.64	—	—	—	—	6	11.10	5	3.31	171	15.62
Bureaucrat xPr	17	4.06	1	1.67	25	3.00	—	—	—	—	1	1.85	4	2.65	38	3.48
Bureaucrat imp	1	.24	—	—	4	.48	—	—	2	13.33	—	—	1	.66	2	.19
	83	19.81	4	6.67	143	17.12	—	—	2	13.33	7	12.95	10	6.62	211	19.29

Table III (continued)

Occupation	W1		F3		W2		D1		P		C		B		v	
	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent
Businessman Pr	39	9.31	14	23.33	154	18.46	—	—	—	—	1	1.85	9	5.96	197	17.97
Businessman xPr	8	1.91	2	3.33	1	.12	—	—	—	—	—	—	1	.66	10	.91
Banker Pr	6	1.43	3	5.00	27	3.23	—	—	—	—	—	—	3	1.99	33	3.02
Banker xPr	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	53	12.65	19	31.66	182	21.81	—	—	—	—	1	1.85	13	8.61	240	21.90
Clergy PrxPr	17	4.06	—	—	33	3.94	—	—	—	—	—	—	—	—	50	4.57
Professional PrxPr	18	4.30	4	6.67	52	6.22	—	—	—	—	—	—	—	—	74	6.77
Misc. PrxPr	9	2.15	—	—	8	.96	—	—	—	—	1	1.85	1	.67	15	1.37
Unknown Pr	44	10.50	5	8.33	100	11.96	—	—	—	—	7	12.95	19	12.58	123	11.25
Unknown xPr	2	.47	—	—	6	.72	—	—	—	—	1	1.85	1	.67	6	.55
	90	21.48	9	15.00	199	23.80	—	—	—	—	9	16.65	21	13.92	268	24.51
	419	100.00	60	100.00	836	100.00	1	100.00	15	100.00	54	100.00	151	100.00	1,094	100.00

Tables IV and V, pp. 769, 770). In all three reigns, few creations were made from the ranks of the Prussian or imperial bureaucracies, a high of 23 per cent being reached under Frederick III.¹⁶ Those bureaucrats who did receive titles were usually in the higher ranks,¹⁷ about 75 per cent of them in the Prussian rather than in the imperial civil service (see Tables V, VII, and VIII, pp. 770, 773, 774). Provincial administrative posts were frequently held by the nobles in Prussian employ, but the most common occupation was the judiciary. The concentration of new Prussian nobles in legal posts is probably attributable to the distaste of the older nobility for serving in this department.¹⁸ The Prussian judiciary was low in both prestige and pay; it was the only branch of the government with a significant number of Jewish officials.¹⁹ William I's creations were usually in their late forties, while his successors favored men well into their fifties (see Table II, p. 763).

Both Williams created about the same number of nobles annually, but the younger ruler, unlike his grandfather, was notorious for the indiscriminate nature of his awards of honors and titles.²⁰ In William II's defense, however, it should be remembered that his reign was a period in which the appetite for insignia and trappings—an affliction one disdainful observer described as "ermine fever"—was widespread.²¹ The last King of Prussia frequently granted patents covering whole branches of families, sometimes resulting in the simultaneous elevation of as many as thirty persons.²² The patents were often issued not to specific individuals but at large to anyone able to prove his descent in that branch. Titles were only part of William's boundless largesse: Red Eagles, *Pour le mérite*, rights of address as *Excellenz* came to be showered with alarming abandon. Many aristocrats of ancient lineage, who rightly felt that the nobility was being vulgarized by a monarch in whom more than one detected strains

¹⁶ Some nobles listed in other professions (e.g., professors and certain doctors of medicine) are, of course, properly bureaucrats in Prussia. Even if their number is added to those of the administrative bureaucrats the percentage is not large.

¹⁷ The division of Prussian bureaucrats into the *Rangklassen* cited in Table VIII was adopted in 1817 and continued, with many alterations, until 1918. The following are the principal officials in each *Klasse*. *Klasse 1*: *Unter Staatssekretäre, Ministerialdirektoren, Oberpräsidenten, Geheime Königliche Oberregierungsräte, Chefpräsidenten* of the *Oberlandesgerichte*. *Klasse 2*: *Vortragende Räte der Ministerien* with the title of *Geheimer Ober Rat, Regierungspräsidenten, Universitätsrektoren, Polizeipräsident* of Berlin, *Oberlandesgerichtspräsidenten*. *Klasse 3*: *Vortragende Räte* with the title of *Geheim Rat, Provinzialsteuerdirektoren, Senatspräsidenten* of the *Oberlandesgerichte, Präsidenten* of the *Landesgerichte, Oberstaatsanwälte, Polizeipräsidenten* (excluding Berlin), *Oberforstmeister*. *Klasse 4*: *Räte, Polizeidirektoren, Landgerichtsdirektoren, Oberlandesgerichtsräte and Landesgerichtsräte, Erste Staatsanwälte, Oberförster, Bauinspektoren*. Subalterns: *Ministerialsekretäre, Gerichtsssekretäre*. For a more complete listing and discussion, see Conrad Bornhak, *Preussisches Staatsrecht* (2 vols., Freiburg, 1889), II, 83-85; and Hermann Lorenz, *Die Amtstitel und Rangverhältnisse der . . . Reichs- und preussischen Staatsbeamten* (Berlin, 1907).

¹⁸ Muncy, *Junker*, 70-73. Note that in Table VII, all judicial personnel are bourgeois raised to the rank of *von*.

¹⁹ Ernest Hamburger, "Jews in Public Service under the German Monarchy," *Leo Baeck Institute, Year Book*, IX (1964), 225-27.

²⁰ William I averaged twenty-five creations per year; William II, twenty-eight. Frederick III came to the throne aware that he was a dying man and granted sixty titles in ninety-nine days.

²¹ [Mme. Morel?], *From an Eastern Embassy: Memoirs of London, Berlin & the East* (Philadelphia, 1920), 161.

²² This was not a common practice with his predecessors; William II was responsible for 85 per cent of the patents of this sort granted during the Empire.

Table IV
Religion, Military Service, Landownership (All Creations)

	Protestant No. Per cent	Catholic No. Per cent	Jew (including those baptized) No. Per cent	Unknown No. Per cent
W ₁	355 84.73	28 6.68	2 .48	34 8.11
F ₃	51 85.00	3 5.00	2 3.33	4 6.67
W ₂	721 86.24	59 7.06	7 .84	49 5.86 = 100%
D	— —	1 100.00	— —	— —
P	14 93.33	1 6.67	— —	— —
C	45 83.35	6 11.10	— —	3 5.55
B	134 88.75	10 6.62	2 1.32	5 3.31
v	934 85.38	72 6.58	9 .82	79 7.22 = 100%

Table IV (continued)

	Military service			Land ownership		
	Yes No.	Per cent	No No.	Per cent	Unknown No.	Per cent
W1	259	61.81	158	37.71	2	.48
F3	14	23.33	46	76.67	—	—
W2	512	61.24	321	38.40	3	.36
D	1	100.00	—	—	—	—
P	6	40.00	9	60.00	1	6.67
C	33	61.11	21	38.89	8	14.81
B	89	58.94	62	41.06	72	47.68
v	656	59.96	433	39.58	812	74.22
					6	.55 = 100%

Table V
Noble's Occupation (All Creations)

Occupation	W ₁		F ₃		W ₂		D		P		C		B		v	
	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent
Agriculture Pr	103	24.58	27	45.00	207	24.82	1	100.00	10	66.67	35	64.85	60	39.75	231	21.11
Agriculture xPr	—	—	—	—	1	.12	—	—	—	—	—	—	—	—	1	.09
	103	24.58	27	45.00	208	24.94	1	100.00	10	66.67	35	64.85	60	39.75	232	21.20
Army Pr	202	48.17	7	11.67	297	35.52	—	—	—	—	10	18.50	48	31.80	448	40.97
Army xPr	3	.72	—	—	4	.48	—	—	—	—	—	—	2	1.32	5	.45
Navy	3	.72	—	—	26	3.11	—	—	—	—	—	—	—	—	29	2.70
	208	49.61	7	11.67	327	39.11	—	—	—	—	10	18.50	50	33.12	482	44.12
Bureaucrat Pr	37	8.83	12	20.00	85	10.17	—	—	—	—	3	5.55	4	2.64	127	11.63
Bureaucrat xPr	6	1.44	—	—	4	.48	—	—	—	—	1	1.85	2	1.32	7	.63
Bureaucrat imp	13	3.10	2	3.33	22	2.63	—	—	5	33.33	3	5.55	7	4.64	22	2.01
	56	13.37	14	23.33	111	13.28	—	—	5	33.33	7	12.95	13	8.60	156	14.27

Table V (continued)

Occupation	W ₁		F ₃		W ₂		D		P		C		B		v	
	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent
Businessman Pr	18	4.32	4	6.66	92	11.00	—	—	—	—	1	1.85	8	5.30	105	9.61
Businessman xPr	1	.24	—	—	3	.36	—	—	—	—	—	—	—	—	4	.36
Banker Pr	5	1.19	3	5.00	18	2.16	—	—	—	—	—	—	3	1.99	23	2.07
Banker xPr	—	—	—	—	4	.48	—	—	—	—	—	—	3	1.99	1	.09
	24	5.75	7	11.67	117	14.00	—	—	—	—	1	1.85	14	9.28	133	12.13
Clergy PrxPr	7	1.67	2	3.33	9	1.08	—	—	—	—	—	—	4	2.64	14	1.26
Professional PrxPr	7	1.67	2	3.33	24	2.87	—	—	—	—	—	—	2	1.32	31	2.79
Academician PrxPr	8	1.91	1	1.67	20	2.39	—	—	—	—	—	—	1	.66	28	2.61
Art/Lit. PrxPr	3	.72	—	—	11	1.32	—	—	—	—	1	1.85	2	1.32	11	.99
Misc. PrxPr	3	.72	—	—	7	.84	—	—	—	—	—	—	5	3.31	5	.45
Unknown Pr	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Unknown xPr	—	—	—	—	2	.17	—	—	—	—	—	—	—	—	2	.18
	28	6.69	5	8.33	73	8.67	—	—	—	—	1	1.85	14	9.25	91	8.28
	419	100.00	60	100.00	836	100.00	1	100.00	15	100.00	54	100.00	151	100.00	1094	100.00

Table VI
Distribution of Military Rank in Prussian Army (Nobles Listed as Army Pr in Table V)

Rank	W ₁		F ₃		W ₂		D		P		C		B		v	
	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent
Field Marshal	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
General	5	2.48	—	—	20	6.73	—	—	—	—	5	50.00	1	2.08	19	4.25
Lt. General	11	5.45	1	14.29	62	20.82	—	—	—	—	—	—	2	4.16	72	16.15
Major General	17	8.42	4	57.13	49	16.48	—	—	—	—	—	—	4	8.32	66	14.81
Colonel	29	14.35	1	14.29	45	15.14	—	—	—	—	—	—	1	2.08	74	16.35
Lt. Colonel	25	12.39	1	14.29	17	5.73	—	—	—	—	—	—	2	4.16	41	9.17
Major	41	20.29	—	—	42	14.20	—	—	—	—	2	20.00	9	18.74	72	15.95
Captain	36	17.82	—	—	30	10.11	—	—	—	—	2	20.00	12	25.02	52	11.55
Lieutenant	37	18.31	—	—	31	10.45	—	—	—	—	1	10.00	17	35.44	50	11.33
Cadet	—	—	—	—	1	.34	—	—	—	—	—	—	—	—	1	.22
Unknown	1	.49	—	—	—	—	—	—	—	—	—	—	—	—	1	.22
	202	100.00	7	100.00	297	100.00	—	—	—	—	10	100.00	48	100.00	448	100.00

Table VII
Distribution of Rank and Department in Prussian Bureaucracy (Nobles Listed as Bureaucrat Pr in Table V)

Rank or Department	W ₁		F ₃		W ₂		D		P		C		B		v	
	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent
State Secretary	3	8.10	5	41.68	10	11.72	—	—	—	—	1	33.34	1	25.00	16	12.64
Klasse 1	4	10.80	4	33.33	17	20.00	—	—	—	—	—	—	1	25.00	24	18.79
Klasse 2	5	13.50	2	16.66	16	18.80	—	—	—	—	—	—	1	25.00	22	17.50
Klasse 3	9	24.40	—	—	11	12.90	—	—	—	—	—	—	—	—	20	15.90
Klasse 4	6	16.20	—	—	6	7.08	—	—	—	—	—	—	—	—	12	9.50
Klasse 5	1	2.70	1	8.33	7	8.26	—	—	—	—	—	—	1	25.00	8	6.32
Subaltern	4	10.80	—	—	2	2.36	—	—	—	—	—	—	—	—	6	4.74
Unknown	4	10.80	—	—	15	17.70	—	—	—	—	1	33.33	—	—	18	13.82
Misc.	1	2.70	—	—	1	1.18	—	—	—	—	1	33.33	—	—	1	.79
	37	100.00	12	100.00	85	100.00	—	—	—	—	3	100.00	4	100.00	127	100.00
Office of Minister- President	3	8.10	—	—	—	—	—	—	—	—	—	—	—	—	3	2.37
Justice	10	27.10	3	25.00	19	22.30	—	—	—	—	—	—	—	—	32	24.98
Municipal Admin.	—	—	—	—	2	2.36	—	—	—	—	—	—	1	25.00	1	.79
Hof	—	—	—	—	3	3.54	—	—	—	—	—	—	—	—	3	2.37
Tax/Finance	6	16.20	1	8.33	7	8.26	—	—	—	—	—	—	1	25.00	13	10.33
Police	2	5.40	—	—	6	7.08	—	—	—	—	—	—	—	—	8	6.32
Provincial Admin.	6	16.20	3	25.00	17	20.00	—	—	—	—	—	—	1	25.00	25	19.91
Agriculture	3	8.10	2	16.67	7	8.26	—	—	—	—	2	66.67	1	25.00	9	7.11
Transportation	—	—	—	—	2	2.36	—	—	—	—	—	—	—	—	2	1.58
Misc.	3	8.10	3	25.00	18	21.12	—	—	—	—	1	33.33	—	—	23	17.92
Unknown	4	10.80	—	—	4	4.72	—	—	—	—	—	—	—	—	8	6.32
	37	100.00	12	100.00	85	100.00	—	—	—	—	3	100.00	4	100.00	127	100.00

Table VIII
Distribution of Rank and Department in Imperial Bureaucracy (Nobles Listed as Bureaucrat imp in Table V)

Rank or Department	W ₁		F ₃		W ₂		D		P		C		B		v	
	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent
Chancellor	1	7.69	—	—	2	9.10	—	—	2	40.00	1	33.34	—	—	—	—
State Secretary	2	15.38	—	—	3	13.65	—	—	—	—	1	33.33	—	—	4	18.20
Ambassador	—	—	1	50.00	5	22.75	—	—	2	40.00	1	33.33	3	42.84	—	—
Geh Ober Reg Rat	1	7.69	—	—	1	4.55	—	—	—	—	—	—	—	—	2	9.10
Geh Reg Rat	1	7.69	—	—	1	4.55	—	—	—	—	—	—	—	—	2	9.10
Rat	1	7.69	—	—	—	—	—	—	—	—	—	—	—	—	1	4.55
Lower FO personnel	5	38.48	—	—	6	27.20	—	—	—	—	—	—	4	57.16	7	31.75
Gov. Alsace-Lorraine	—	—	—	—	1	4.55	—	—	1	20.00	—	—	—	—	—	—
Misc.	2	15.38	1	50.00	3	13.65	—	—	—	—	—	—	—	—	6	27.30
	13	100.00	2	100.00	22	100.00	—	—	5	100.00	3	100.00	7	100.00	22	100.00
Chancellor's Office	3	23.07	—	—	3	13.65	—	—	2	40.00	1	33.33	—	—	3	13.65
Foreign Office	7	53.86	1	50.00	13	59.05	—	—	2	40.00	2	66.67	7	100.00	10	45.40
Tax/Finance	1	7.69	—	—	1	4.55	—	—	—	—	—	—	—	—	2	9.10
Transportation	1	7.69	—	—	1	4.55	—	—	—	—	—	—	—	—	2	9.10
Justice	—	—	1	50.00	1	4.55	—	—	—	—	—	—	—	—	2	9.10
Alsace-Lorraine	—	—	—	—	2	9.10	—	—	1	20.00	—	—	—	—	1	4.55
Post Office	1	7.69	—	—	—	—	—	—	—	—	—	—	—	—	1	4.55
Misc.	—	—	—	—	1	4.55	—	—	—	—	—	—	—	—	1	4.55
	13	100.00	2	100.00	22	100.00	—	—	5	100.00	3	100.00	7	100.00	22	100.00

of the parvenu, acutely resented William's capricious favors.²³ Some of William's creations were not kindly treated by bearers of less recent titles, who regarded as sorry masquerade the decoration of tradesmen's sons with noble insignia. William II's competent Civil Cabinet Chief, Friedrich Lucanus, the son of a druggist, was given a *von* and subjected thereafter to the sobriquet *Der Apotheker* by members of the older aristocracy.²⁴ Titles awarded by other sovereigns to men from business backgrounds were no better received.²⁵

By the end of the Empire, even the dignity of prince was threatened with a decline in reputation. William I considered the princely rank appropriate only for men possessing great fortunes that they were willing to entail so that future holders of the title could maintain themselves in a state suitable to their exalted rank.²⁶ He was consequently very sparing in granting it; his only such award during the Empire went in 1871 to Bismarck. From his father, Bismarck had inherited an estate in Pomerania of less than 2,000 hectares, but William I, on elevating his minister to count in 1867, had seen to it that Bismarck's landed position matched his new dignity by giving him 400,000 thalers (about £60,000 in the currency of the day), directing that it be used to purchase an entailed estate. Bismarck invested this sum, together with 100,000 thalers of his own savings, in the acquisition of Varzin in Pomerania, an estate of 8,125 hectares. In 1871, on being elevated to prince, Bismarck was given the Sachsenwald in Lauenburg, some 8,114 hectares in extent. Bismarck simultaneously purchased Friedrichsruh, located in the midst of the forest, a holding that, with later additions,

²³ *Denkwürdigkeiten des General-Feldmarschalls Alfred Grafen von Waldersee*, ed. Meisner, II, 220, 424, 426; *Das Tagebuch der Baronin Spitzemberg, geb. Freiin v. Varnbüler: Aufzeichnungen aus der Hofgesellschaft des Hohenzollernreiches*, ed. Rudolf Vierhaus (2d rev. ed., Göttingen, 1960), 392-93, 530; Ludwig Raschdau, *In Weimar als preussischer Gesandter: Ein Buch der Erinnerungen an deutsche Fürstenhöfe, 1894-1897* (Berlin, 1924), 107, 110-12, 119, 184-85; Alexander Prince Hohenlohe, *Aus meinem Leben* (Frankfurt a.M., 1925), 327-28; Colmar Baron von der Goltz, *Denkwürdigkeiten* (Berlin, 1929), 297. William II's most absurd innovation with respect to honors was the striking of the Kaiser William I centenary memorial medal in 1897. Every Prussian soldier and bureaucrat was eligible to receive this decoration, and it was therefore popularly referred to as the *Zeitgenossenmedaille*. (See Eugen von Jagemann, *Fünfundsiebzig Jahre des Erlebens und Erfahrens (1849-1924)* [Heidelberg, 1925], 143.) For William II as parvenu, see [Edgar Vincent] Viscount D'Abernon, *The Diary of an Ambassador* (3 vols., Garden City, N. Y., 1929-31), II, 210-11; and Maurice F. Egan, *Ten Years near the German Frontier: A Retrospect and a Warning* (New York, 1919), 59, for Prussian critics; complaints from Bavarians can be found in Philipp Count zu Eulenburg to Friedrich von Holstein, Feb. 9, 1892, Eulenburg Papers, XVII, Bundesarchiv, Koblenz; Marie von Bunsen, *Zeitgenossen die ich erlebte, 1900-1930* (Leipzig, 1932), 93; and in Alexander Hohenlohe, *Aus meinem Leben*, 360; see also Jules Huret, *En Allemagne: Berlin* (Paris, 1909), 89-90.

²⁴ On Lucanus, also known to Eulenburg and others as "Look anus," see Holstein to Eulenburg, Sept. 23, 1895, Eulenburg Papers, XXXVII; notice by Eulenburg, Oct. 22, 1896, *ibid.*, XLIV; Eulenburg to Holstein, Feb. 1, 1897, *ibid.*, XLV; see also *Erinnerungen und Gedanken des Botschafters Anton Graf Monts*, ed. Karl Friedrich Nowak and Friedrich Thimme (Berlin, 1932), 380; and Bogdan Count von Hutten-Czapski, *Sechzig Jahre Politik und Gesellschaft* (2 vols., Berlin, 1935), I, 311.

²⁵ Ambassador Prince Münster, merely a count (though one of great ancestry) until William II elevated him, complained that a young secretary in his embassy in Paris, Wilhelm Schoen, the son of a Worms merchant grown rich in hides, "smelled of leather" and therefore, presumably, was unworthy of the *von* conferred on him by the Grand Duke of Hesse. Later Schoen acquired a baronage from the same source, an elevation that exposed him to ridicule. (See Bernhard Prince von Bülow, *Denkwürdigkeiten* [4 vols., Berlin, 1930-31], I, 395; III, 31.)

²⁶ *The Correspondence of William I. & Bismarck*, tr. A. J. Ford (2 vols., New York, 1903), I, 157-58.

comprised 1,234 hectares.²⁷ The two individuals elevated to princely rank by Frederick III held considerable property; one of them, Prince Solms-Baruth, owned an entail of 30,648 hectares in Brandenburg, one of the largest holdings in Prussia.²⁸ Under William II, however, the monetary requirement for princes slipped somewhat in that he elevated two men (Chancellor Bülow and Philipp Eulenburg) whose estates were modest and who had no great personal fortunes derived from other sources. The King's relaxation of standards did not escape aristocratic criticism in the case of Eulenburg, whose elevation occasioned much disapproval in Berlin's *erste Gesellschaft*.²⁹ Bülow owned less land but possessed more capital than Eulenburg; the Chancellor's prestige and popularity apparently spared him reproach.

At the same time William II stubbornly insisted on awarding higher titles to men who did not want them but who were not always able to resist the King's pleasure. The reasons for this lack of enthusiasm varied. Some were not anxious to owe anything to a monarch they did not like; some felt they could not afford to maintain themselves on the more elaborate scale an advance would require; others preferred to be distinguished as ancient *von*'s or barons rather than to be the most recent of the counts or princes.³⁰

A differentiation of the new creations and advancements made not by reign but by random spans of time indicates the possibility that changes in the character of the nobility perhaps reflected transformations in Prussian society as well as differences in royal personalities. The spans, chosen arbitrarily, that were used

²⁷ Alfred Vagts, "Bismarck's Fortune," *Central European History*, I (Sept. 1968), 209; see also *Correspondence of William I. & Bismarck*, I, 85; Ferdinand Philipp, "Die Erinnerungen von Bismarcks Anwalt," *Süddeutsche Monatshefte*, XXIV (Feb. 1927), 387; and Otto von Bismarck, *Die Gesammelten Werke* (15 vols., Berlin, 1923-33), VIII, 532. When the Iron Chancellor resigned in 1890, William II declared his intention of making Bismarck Duke of Lauenburg, the title to be accompanied by a dotation to cover the additional costs of living involved. Bismarck refused, however, declaring that without the dotation he was too poor to become a duke, and with it he would have let himself be bought off. (See Szögyényi to Vienna, Mar. 25, 1890, Preussen III, CXXXVIII [*Berichte*], HHStA; Eulenburg diary, Mar. 18, 1890, Eulenburg Papers, X.)

²⁸ This figure is given in the *Gothaischer Genealogischer Hofkalendar nebst diplomatisch-statistischen Jahrbuch* (Berlin, 1885), 201. Solms's holdings paled beside the great latifundia held by the princes of Pless (70,139 hectares) and of Hohenzollern-Sigmaringen (60,000 hectares). (See *Handwörterbuch der Staatswissenschaften*, ed. J. Conrad et al. [3d rev. ed., 8 vols., Jena, 1909-11], VI, 411-12.)

²⁹ See *Tagebuch der Baronin Spitzemberg*, ed. Vierhaus, 392-93; Chlodwig Prince zu Hohenlohe-Schillingsfürst, *Denkwürdigkeiten der Reichskanzlerzeit* (Stuttgart, 1931), 554; and Hutten-Czapski, *Sechzig Jahre*, I, 249-50. There is, however, considerable material in Volume LXIX of the Eulenburg Papers that indicates William II's interest in determining the financial position of the Dohna family before awarding one or more of its members the title of prince.

³⁰ Hans von Tresckow, *Von Fürsten und anderen Sterblichen: Erinnerungen eines Kriminalkommissars* (Berlin, 1922), 173-74; *Tagebuch der Baronin Spitzemberg*, ed. Vierhaus, 392-93, 405; Axel Count Schwering (pseud.), *The Berlin Court under William II* (London, 1915), 160; Hellmuth von Gerlach, *Erinnerungen eines Junkers* (Berlin, n.d.), 14; Jaeger, *Unternehmer in der deutschen Politik*, 259-60; Philipp, "Erinnerungen," 390. For Chancellor Theobald von Bethmann Hollweg's refusal to be elevated to count, see Szögyényi to Foreign Minister Count Aehrenthal, June 11, 1911, Preussen III, CLXIX (*Varia*), HHStA. Chancellor Leo von Caprivi's unhappiness at his unexpected promotion to count is reported in Szögyényi to Kálnoky, Dec. 22, 1891, CXLI (*Varia*), *ibid.* One prominent industrialist declined a baronage offered by Frederick III on the grounds that as Baron von Krupp his business might suffer. (See Robert Baron Lucius von Ballhausen, *Bismarck-Erinnerungen* [Stuttgart, 1921], 450.) For refusals of offers of *von*, see Morsey, *Oberste Reichsverwaltung*, 247-48.

for this purpose were 1871-1879, 1880-1893, 1894-1905, 1906-August 4, 1914, and August 5, 1914-November 9, 1918 (see Table IX, pp. 778-80).

Such a differentiation shows that the percentage of Roman Catholic nobles increased significantly after the end of the Kulturkampf (1879) and that the percentage of Jewish creations rose sharply in the decade preceding the outbreak of the First World War. In parentage an almost steady increase in the percentage of bankers, businessmen, and professional men can be noted, while the number of fathers in military service shows an almost constant decline. Among the nobles themselves there is an almost steady fall in the percentage of army officers, and there is an almost constant increase in the percentage of naval, business, banking, and professional figures. Those nobles created during World War I were less likely than their peacetime predecessors to have either aristocratic fathers, mothers, or wives. Insufficient data prevent a conclusive description of the professional occupation of the fathers of these wartime creations. Titles went less to those in the military than to those in agriculture, and the number of bureaucrats honored showed a marked increase. It should be emphasized, however, that while more nobles, and more nobles' fathers, were involved in banking, business, or professions as we near 1918, their number never approached those drawn from agrarian, military, or bureaucratic pursuits.

If the new or newly advanced nobles are differentiated by rank rather than by creator, the following conclusions can be drawn from the data. Higher titles tended to go to men of ancient aristocratic lineage, both paternal and maternal, and higher ranking nobles were more likely to marry noblewomen (see Table X, pp. 782-83). Awards of baron or higher were rarely (26 of 221 awards) made to men with bourgeois fathers, and then usually only after they had first been granted the ennobling *von*. As one ascends through the degrees of nobility one encounters an increasing concentration of men whose fathers' livelihoods were derived from agriculture (see Table III, pp. 765-66). Few nobles, especially those in the ranks above *von*, had bureaucratic fathers. Only in the rank of *von* could the nobles' family backgrounds be described as diverse.

The men granted titles, when differentiated by rank, show an occupational differentiation similar to that which distinguished their fathers. Again, as we ascend through the grades of the nobility, the percentage of creations of men engaged in agriculture rises without exception. The opposite curve occurs in the case of men who were professional soldiers. A higher proportion of *von*'s were soldiers or sailors; as one moves up in the nobility the percentage of military figures decreases (see Table V, pp. 770-71). Military officers elevated to the rank of count were more likely to be generals, while honors of baron and *von* were more evenly spread through the ranks (see Table VI, p. 772). The proportion of bureaucrats in the Prussian civil service is never great and disappears entirely in the ranks of prince and duke (see Table V, pp. 770-71). High titles usually went

Table IX
Religion, Parentage, Wives, Fathers' and Nobles' Occupations, etc. (All Creations)

	1871-79 No. Per cent 199 creations	1880-93 No. Per cent 371 creations	1894-1905 No. Per cent 319 creations	1906-14 No. Per cent 379 creations	1914-18 No. Per cent 47 creations			
Religion: Prot	173	86.94	313	84.37	324	85.49	32	68.05
RC	7	3.52	29	7.82	30	7.92	2	4.26
Jew	1	.50	2	.53	1	.31	—	—
Unk	18	9.04	27	7.28	11	3.45	13	27.69
Military	143	71.86	192	51.75	189	59.25	237	62.27
service: No	54	27.14	179	48.25	129	40.44	140	37.20
Unk	2	1.00	—	—	1	.31	2	.53
Noble father	43	21.61	78	21.02	33	10.34	29	7.65
Noble mother	57	32.02	70	20.59	54	17.88	66	18.49
Noble wife	65	32.66	104	28.02	90	28.21	91	24.01
Father's occupation:								
Agriculture Pr	50	25.10	109	29.38	78	24.50	89	23.58
Agriculture xPr	4	2.01	2	.54	5	1.57	—	—
Army Pr	32	16.08	42	11.32	29	9.09	42	11.10
Army xPr	8	4.02	16	4.32	4	1.24	4	1.04
Navy	1	.50	—	—	5	1.57	—	—
Bureaucrat Pr	29	14.56	51	13.75	51	16.00	48	12.70
Bureaucrat xPr	12	6.03	10	2.70	11	3.44	10	2.60
Bureaucrat imp	2	1.01	1	.27	1	.31	—	—
Businessman Pr	10	5.03	53	14.29	58	18.20	77	20.42
							9	18.21

Table IX (continued)

Businessman xPr	5	2.52	5	1.35	1	.31	—	—	—
Banker Pr	4	2.01	5	1.35	11	3.44	16	4.16	—
Banker xPr	—	—	—	—	—	—	—	—	—
Clergy PrxPr	6	3.02	14	3.78	10	3.13	20	5.20	—
Professional PrxPr	5	2.52	20	5.40	24	7.52	22	5.72	6.35
Misc.	5	2.52	5	1.35	3	.93	4	1.04	—
Unknown Pr	23	11.56	37	9.93	25	7.84	46	12.18	18 39.12
Unknown xPr	3	1.51	1	.27	3	.91	1	.26	—
Nobles' occupation:									
Agriculture	35	17.58	123	33.15	67	21.07	98	25.64	14 29.77
Agriculture xPr	—	—	—	—	1	.31	—	—	—
Army Pr	119	59.80	131	35.30	124	39.00	121	32.00	11 23.37
Army xPr	1	.50	3	.81	1	.31	2	.52	—
Navy	2	1.00	1	.27	9	2.79	15	3.96	2 4.26
Bureaucrat Pr	9	4.53	43	11.59	34	10.66	40	10.58	8 17.04

Table IX (continued)

Bureaucrat xPr	4	2.01	2	.54	2	.62	2	.52	—	—
Bureaucrat imp	7	3.52	11	2.96	12	3.72	6	1.56	1	2.13
Businessman Pr	10	5.03	19	5.12	37	11.60	42	11.11	6	12.78
Businessman xPr	—	—	2	.54	1	.31	1	.26	—	—
Banker Pr	3	1.51	5	1.35	6	1.86	11	2.86	1	2.13
Banker xPr	—	—	1	.27	2	.62	1	.26	—	—
Clergy PrxPr	4	2.01	7	1.89	4	1.24	3	.78	—	—
Professional PrxPr	—	—	8	2.16	8	2.48	17	4.75	—	—
Misc.	2	1.00	3	.81	3	.93	1	.26	1	2.13
Unknown Pr	—	—	—	—	—	—	2	.52	—	—
Unknown xPr	—	—	—	—	—	—	—	—	—	—
Academician	3	1.51	8	2.16	6	1.86	10	2.60	2	4.26
Arts/Letters	—	—	4	1.08	2	.62	7	1.82	1	2.13

to men in high offices (see Table VII, p. 773). The judiciary, avoided by the old aristocracy, was the principal department of the government from which bourgeois bureaucrats were elevated into the inferior ranks of the nobility. Awards to imperial bureaucrats were rare; 57 per cent of them went to men working in the Foreign Office (see Table VIII, p. 774).

One vital question must unfortunately remain inadequately answered: why were these particular men singled out for royal favor, and to what did they owe their new status? Neither Houwald, Gritzner, the *Gotha*, nor other annuals treating the nobility say much more than that occasionally, for example, a soldier received his title "for good and true service in both war and peace." Sometimes the biographical data reveal the reason for the creation. The grant of a title to a commoner on the day of his marriage to a noble lady probably was not a coincidence. Sometimes the new noble is recognizable as an intimate of the King, or as a civil servant particularly valued by his royal master. Both Gritzner and Houwald abound in names of old friends, or favorites,³¹ or trusted counselors of William I and his successors, drawn into or elevated within the nobility. Some creations were attributable to impulsive acts of generosity; some went to old civil servants on the eve of retirement (or to their sons, if, presumably, the fathers declined the honor). Others were awarded to descendants in the female line of titles extinguished in the male line or to landlords inheriting or creating entails.

Acts of conspicuous philanthropy by wealthy bourgeois also occasioned the bestowal of titles, and indeed the eleemosynary instinct seems to have been considerably heightened by the prospect that good works might be rewarded by noble patents. William I admitted to Bismarck that grants to charities—or to the House of Hohenzollern—might be reciprocated by grants of nobility, and under William's grandson the connection between the two became transparent.³² Robert Count Zedlitz-Trützschler, who spent seven years at court as one of William II's *Hofmarschälle* and who was therefore in a position to know, estimated in 1904 that in the preceding fifteen years almost one million marks had been contributed annually to good works in return for titles and other royal favors.³³ An illustrative anecdote, given some currency in Berlin society in 1907, concerned the efforts of Fritz Friedländer-Fuld to obtain a *von*. Friedländer, who was immensely wealthy from Silesian coal and iron, and his wife were very ambitious

³¹ William II introduced more favorites into the nobility than did his predecessors. His creations included several personal physicians and military adjutants, the architect Ernst Ihne and the court painter Adolf Menzel, both of whose works the last Kaiser greatly admired; Count Douglas, a rich industrialist often seen at court, and "Phili" Eulenburg, at the time of his elevation to prince in 1900 a confidant.

³² *Correspondence of William I. & Bismarck*, I, 160-61. In 1877 William I granted the title of count to Adolf von Schack after Schack, according to Eulenburg, promised to will his valuable art collection to the Prussian royal house. When he died in 1894, the treasures passed into the possession of the Hohenzollerns. (Eulenburg to William II, Apr. 16, 1894, Eulenburg Papers, XXIX.) See also Rogge, *Holstein und Harden*, 179 n.3.

³³ Robert Count Zedlitz-Trützschler, *Zwölf Jahre am deutschen Kaiserhof* (Berlin, 1924), 75-77. See also Friedrich Schmidt-Ott, *Erlebtes und Erstrebtes 1860-1950* (Wiesbaden, 1952), 82-83; *Tagebuch der Baronin Spitzemberg*, ed. Vierhaus, 530.

Table X
Mothers, Fathers, and Wives of Noble Birth, Blood Relationship to Other Creations, Age (All Creations for Whom Relevant Biographical Information Is Complete)

von's (1,094 creations)		Barons (151 creations)		Counts (54 creations)		Princes (15 creations)		Dukes (1 creation)	
No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent	No.	Per cent
Parentage: information complete for 1,022 cases or 93.42% of all creations		Parentage: information complete for 129 cases or 85.43% of all crea- tions		Parentage: information complete for 44 cases or 81.48% of all crea- tions		Parentage: information complete for 15 cases or 100% of all crea- tions		Parentage: information complete for 1 case or 100% of all crea- tions	
Noble mother	131	Noble mother	78	Noble mother	28	Noble mother	12	Noble mother	1
Noble father	0	Noble father	126	Noble father	44	Noble father	15	Noble father	1
Both parents noble	0	Both parents noble	78	Both parents noble	28	Both parents noble	12	Both parents noble	1
One parent noble	131	One parent noble	48	One parent noble	16	One parent noble	3	One parent noble	0
Neither parent noble	891	Neither parent noble	3	Neither parent noble	0	Neither parent noble	0	Neither parent noble	0
Total	1022	Total	129	Total	44	Total	15	Total	1
Marriage: in- formation complete for 1,033 cases or 94.42% of all creations		Marriage: in- formation complete for 144 cases or 95.36% of all creations		Marriage: in- formation complete for 54 cases or 100% of all creations		Marriage: in- formation complete for 15 cases or 100% of all creations		Marriage: in- formation complete for 1 case or 100% of all creations	
Married to noble wives before or with- in 6 months of creation	242	Married to noble wives before or with- in 6 months of creation	64	Married to noble wives before or with- in 6 months of creation	38	Married to noble wives before or with- in 6 months of creation	12	Married to noble wives before or with- in 6 months of creation	1
	23.43		44.44		70.37		80.00		100.00

Table X (continued)

von's (1,094 creations)	Barons (151 creations)	Counts (54 creations)	Princes (15 creations)	Dukes (1 creation)
Noble relatives: information complete for all cases brothers, step-brothers, fathers, step-fathers, sons, uncles, or nephews also created, 1871-1918 339 Number reporting one or more noble relationships listed above	Noble relatives: information complete for all cases brothers, step-brothers, fathers, step-fathers, sons, uncles, or nephews also created, 1871-1918 86 Number reporting one or more noble relationships listed above	Noble relatives: information complete for all cases brothers, step-brothers, fathers, step-fathers, sons, uncles, or nephews also created, 1871-1918 8 Number reporting one or more noble relationships listed above	Noble relatives: information complete for all cases brothers, step-brothers, fathers, step-fathers, sons, uncles, or nephews also created, 1871-1918 2 Number reporting one or more noble relationships listed above	Noble relatives: information complete for all cases brothers, step-brothers, fathers, step-fathers, sons, uncles, or nephews also created, 1871-1918 0 Number reporting one or more noble relationships listed above
Average age 52.93	Average age 47.29	Average age 55.57	Average age 60.62	Average age 53.00

for social preferment. He approached Chancellor Bülow and offered to contribute 500,000 marks to some, perhaps to any, charity in return for a title. Bülow countered that the sum would have to be doubled to guarantee a successful entreaty with William II. Friedländer agreed, and Bülow reported the transaction to the Kaiser, who exclaimed delightedly, "Haben Sie nicht noch mehr von der Sorte Menschen?" The aspiring candidate got his *von*.³⁴

In many cases a man desiring a title could apply to the king, sometimes directly but usually through some influential noble intermediary. If the would-be noble was from a family whose claim to a patent of nobility was cloudy but possible, he could ask for a confirmation or recognition of his descent and right to bear the title. If, however, no pretension could be made to an ancient line, discreet overtures might be initiated to bring the desire to be ennobled, or advanced, to the ruler's attention. Saxe-Coburg had long been notorious, and Bavaria not above suspicion, for trafficking in patents of nobility, and by the turn of the century some Prussian aristocrats were convinced that venality was playing an increasingly conspicuous role in such affairs in Berlin as well. This unsavory discovery became particularly painful when it was found that some nobles, of excellent connections but penurious means, had gathered fees for serving as channels of influence for those in pursuit of titles and other crown favors.³⁵

Of the 1,315 awards considered here, the reason for the grant of 696 (just under 53 per cent) can be assigned with some certainty. A total of 257 was bestowed for miscellaneous but clearly discernible reasons, 63 for military, and 5 for bureaucratic service, with this fact declared in their patents. There were 116 confirmations of obscure Prussian titles or recognitions in Prussia of foreign ones. Considerations of landownership prompted 13 awards, and at least another 11 were likely occasioned by the same factor. It is probable that 39 bureaucrats of *Klasse 1* or higher category received their titles because of the sovereign's personal acquaintance with them through the conduct of affairs of state.

A total of 192 titles to military figures can be accounted for as follows. Of 169 generals receiving titles, 85 (50.30 per cent) were honored on the occasion of their being transferred from active service to the reserves (*zur Disposition*). Another seven entered the reserves within a year of receiving titles, the award thus presumably being in anticipation of their forthcoming change in status. Because of the extensive acquaintance that both William I and William II had in military circles, it seems likely that they knew personally the 96 generals and admirals on active service receiving titles and that their titles resulted from the

³⁴ Tresckow, *Fürsten*, 173; *Tagebuch der Baronin Spitzemberg*, ed. Vierhaus, 456-57.

³⁵ *Denkwürdigkeiten des General-Feldmarschalls Alfred Grafen von Waldersee*, ed. Meisner, II, 220; Marie von Bunsen, *Lost Courts of Europe* (New York, 1930), 99; Hohenlohe, *Aus meinem Leben*, 327-28; Vagts, "Bismarck's Fortune," 223, n. 88, in addition to the references in notes 33 and 34, above. For Prussian nobles' traffic in parliamentary votes, see Paul Count Vasili (pseud., perhaps for Catherine Princess Radziwill), *La société de Berlin* (Paris, 1884), 21-22. Sales of titles in other German states are mentioned in Jagemann, *Fünfundseibzig Jahre*, 148; and Johann-Heinrich Graf Bernstorff, *Memoirs*, tr. Eric Sutton (New York, 1936), 64.

King's satisfaction with their martial capacities or personal characteristics. The reason for the honors to lower officers is more difficult to determine, but four promotions went to bourgeois serving in crack regiments of almost exclusively aristocratic character.

For the remaining 619 creations there is simply no clear evidence.

Finally, the data provide information on patterns of landownership.³⁶ The differentiation here will be made in terms of the different sorts of legal character applied to landed property, and the rank in the nobility awarded the newly created or newly advanced landowning nobles. My concern in dealing with the landlord nobles is to determine to what extent these men composed a settled landed class. How many had fathers who had also been landlords, especially landlords on the same estates? Next, can a connection between various ranks and various sorts and sizes of landed property be detected? Does a relationship exist between the acquisition of landed estates, or an alteration in the legal character of land already owned, and the award of a title? To what extent were conditions regarding retention of land and preservation of its legal character imposed on patents of nobility granted to landlords?

A number of legally defined types of landownership existed in Prussia prior to 1918, but the form examined here is the *Fideikommiss* (entailed estate). In the *Gotha* and other sources on which data regarding the nobility depend, landed property is, unfortunately, more often referred to descriptively than legally. Thus, for example, many lords will be described as "Herr auf Nieder-Bockhorn" or some such estate name, information that tells us nothing about the legal character of Nieder-Bockhorn. Some estates so described were discovered to be *Fideikomnisse* and were tabulated as such; otherwise they are hereafter referred to as *Herrschaften*.

In Prussian law a *Fideikommiss* was constituted when the owner of lands with an annual yield of at least 7,500 marks or of capital in the sum of 30,000 marks made a declaration prescribing the manner in which the use of said property, undiminished and unencumbered, was to devolve upon his heirs.³⁷ The arrangement was calculated to preserve the prestige (*Ansehen*) of the family and could be terminated only when it failed to attain this objective, when the family died out, or when all the affected participants agreed to the dissolution of the *Fideikommiss*.³⁸

³⁶ Urban real estate is not considered.

³⁷ Only 2 of the 172 Prussian *Fideikomnisse* belonging to men elevated during the Empire were *Geldfideikomnisse*, the others being composed of land. The law provided exceptions through which a *Fideikommiss* could be reduced in size or capital. What these exceptions are is not clear, but the advantage taken of them can be noted in the *Statistisches Jahrbuch für den preussischen Staat*, 1913 (Berlin, 1914), 82-83.

³⁸ *Handwörterbuch der Staatswissenschaften*, ed. Conrad et al., IV, 104-106.

During the course of the Empire, 170 titles were conferred on men who owned an equal number of landed *Fideikomnisse* in Prussia. The hectarage is known for 103 of these estates, their average size being 2,496 hectares (see Table XI, below).³⁹ Considerably more *Fideikomnisse* were located in the provinces of Silesia, Posen, and Pomerania, while fewer, but on an average more extensive, entails were found in East Prussia, West Prussia, and Brandenburg—all areas east of the Elbe. The *Fideikomnisse* belonging to the nobles created or promoted during the Empire constituted 13 per cent of the total number of such estates in existence in 1912 and were rather larger (2,496 versus 1,920 hectares) than the average of all *Fideikomnisse* in Prussia in that year.⁴⁰

Three out of every four holders of *Fideikomnisse* ennobled during the Empire owed their creations to William II, who was more inclined to elevate such agrarian figures than were his predecessors (see Table XII, p. 787). Entails belonging to the new creations were significantly larger in William II's reign than in that of his grandfather, and, unlike William I, he made numerous awards to holders of *Fideikomnisse* in Posen (see Tables XII and XIII, pp. 787, 788). William I was much more insistent than the other two monarchs that the continuing possession

Table XI
Fideikommiss Holdings (All Creations)

Province	No. of Fkm held by new creations	No. of these for which hectarage is available	Total hectarage of these Fkm	Average hectarage of these Fkm
East Prussia	9	3	6,946	2,315
West Prussia	12	10	21,976	2,198
Posen	27	17	45,208	2,659
Pomerania	24	20	58,704	2,935
Silesia	30	23	63,881	2,777
Brandenburg	10	8	45,944	5,743
Saxony	8	4	4,266	1,067
Rhineland	10	5	3,454	691
Westphalia	2	2	357	179
Hanover	9	4	4,971	1,243
Hesse	7	4	1,510	380
Schleswig-Holstein	6	4	4,226	1,057
Unknown	16	1	652	652
Total	170	105	262,095	2,496

³⁹ The nobles owned an additional eight *Fideikomnisse* outside Prussia, one being in Anhalt, six in the Mecklenburgs, and one in an unknown location.

⁴⁰ *Statistisches Jahrbuch für den preussischen Staat*, 1913, 83.

Table XII
Fideikommiss Holdings (All Creations)

	No. Fkm- owning nobles created	No. for which hectar- ages of estate known	Total no. ha.	Ave ha. per Fkm	% with conditional patents	Location of Fkm													
						EP	WP	Pos	Pom	Sil	Bbg	Sax	Rhld	Wfal	Hano	Hess	SH	Unk	
W1	39	27	54,782	2,029	58.97	1	2	1	7	10	3	1	2	1	1	1	3	6	
F3	5	—	—	—	40.00	—	—	2	—	—	—	2	—	—	1	—	—	—	
W2	126	78	207,313	2,658	34.13	8	10	24	17	20	7	5	8	1	7	6	3	10	
D	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	
P	9	4	56,924	14,231	75.00	1	—	1	—	2	—	—	1	—	2	—	1	1	
C	35	30	109,527	3,651	81.58	—	4	2	12	7	2	2	1	—	1	—	2	2	
B	38	22	27,307	1,241	73.68	2	1	2	6	7	1	2	2	1	4	3	2	5	
v	88	49	68,337	1,395	.59	6	7	22	6	14	7	4	6	1	2	4	1	8	

Table XIII
Average Hectarage of *Fideikomisse* Owned by Men Created or Advanced, 1871-1918 (All Creations)

	EP	WP	Pos	Pom	Sil	Bbg	Sax	Rhld	Wfal	Hano	Hess	SH	Unk
W ₁	—	2,887	—	2,411	1,743	3,553	2,390	1,570	—	—	135	1,090	—
F ₃	—	—	—	—	—	—	—	—	—	—	—	—	—
W ₂	2,315	2,025	2,659	3,284	3,443	6,337	625	471	357	1,243	462	1,024	652
P	—	—	—	8,125	16,731	30,648	—	—	—	1,420	—	—	—
C	—	2,646	8,732	3,636	4,118	3,193	1,110	1,570	—	400	—	1,682	—
B	—	1,544	2,005	1,000	1,431	—	2,390	1,000	217	2,270	135	1,024	652
v	2,315	2,082	1,906	976	1,039	1,619	388	295	140	881	625	497	—

of a title depend on retention of the entail by the noble created or by his descendants. This is undoubtedly because the great majority of titles (32 of 39, or 82 per cent) he awarded to holders of *Fideikomnisse* were those of baron, count, or prince. Retention of the estate was considered necessary for the preservation of the dignity of such superior titles. On the other hand, only 2 of Frederick III's 5 and 48 of William II's 126 (38 per cent) creations who held *Fideikomnisse* were at the level of baron or above.

Of the 170 nobles who held *Fideikomnisse*, 44 per cent had inherited the lands comprising these entails from their fathers, 2 per cent from their wives, 3 per cent from their mothers, 2 per cent from extinguished branches of the family, and 1 per cent from stepfathers. The nature of the acquisition of the rest is unknown; some doubtless were obtained by purchase, others by inheritance. Four out of every 10 creations who held *Fideikomnisse* had conditions imposed in their patents, most of them limiting the title's transmission through primogeniture and possession of the estate. Restriction of inheritance to male heirs in possession accounted for most of the remaining conditions. When the future of a title was dependent on the retention of an entail, the entail was rather larger (averaging 2,802 versus 2,496 hectares) than the average size of all *Fideikomnisse* held by the new or newly advanced nobles. Generally the higher a noble's rank the more likely he was to possess a Prussian *Fideikommiss*, to hold one larger in size than those belonging to men of inferior title, and to have his patent qualified by a condition concerning the devolution of the entail (see Table XII, p. 787).

It is difficult to determine whether or not the new nobles were themselves responsible for the establishment of the *Fideikomnisse*. What we do know is that of the 170 creations who held *Fideikomnisse*, 49 (29 per cent) were the *Stifter*, that is, the creators of the entails.⁴¹ In the case of another 37 (22 per cent), the entails had already been established, in most cases by the holders' fathers. Who first established the other *Fideikomnisse* cannot be determined. The information available is not sufficiently complete to warrant any conclusion as to the correlation between date of award of title and date of establishment (*Stift*) or inheritance of a *Fideikommiss*. For only 35 of the 170 entails do we know the date of the *Stift* or of inheritance, but 18 of these 35 were created or inherited within two years of the holder's being granted a title (see Table XIV, p. 790). The evidence is admittedly thin, but it encourages speculation that one of the inducements to establish a *Fideikommiss* was the prospect of the prompt award of a title. This conclusion is strengthened by the fact that 13 titles granted from 1871 to 1918 were made conditional on the *future* establishment of an entail. Besides, the Prussian royal house evidenced a clear interest in maintaining a settled landed class. William I was much in favor of the establishment of *Fideikomnisse* and frequently exempted those who founded entails from the customary registration

⁴¹ Of these, one-third had inherited from their fathers the estates from which the entails were created.

fees.⁴² It was, moreover, the custom of the kings of Prussia to signify their pleasure at the retention of estates (presumably of all legal types) by the same family for a period of years by granting decorations to its members on the anniversary of their acquisition of the property.⁴³ Finally, in Bavaria, the crown proffered titles to men acquiring estates (whether specifically *Fideikomisse* is, however, not clear).⁴⁴

There is also the category of estates described above as *Herrschaften*, landed estates that were not entailed. Houwald and Gritzner report some 198 nobles whose property is so described, with 191 of these estates located in Prussia. As already noted, the properties named as *Herrschaften* in the *Gotha* and elsewhere have in some instances been discovered to be *Fideikomisse* and have been tabulated under that category of landownership. There is some evidence, however, that few if any of the 191 estates here counted as *Herrschaften* were in fact *Fideikomisse*. First, the average hectarage of these estates is considerably smaller than the lordly *Fideikomisse* (881 as opposed to 2,496 hectares) (see Tables XV and XVI, pp. 791, 792). Second, conditions regarding transmission of titles, a prominent feature of patents granted to nobles who held *Fideikomisse*, were almost never (10 cases out of 191) employed in the case of holders of *Herrschaften*. This is probably due to the fact that an overwhelming number of holders of *Herrschaften* were ennobled only at the *von* rank, while holders of *Fideikomisse* were frequently granted higher titles. Like the holders of *Fideikomisse*, however, slightly over 40 per cent of the holders of *Herrschaften* had inherited these estates from their families. In both types of estate higher ranking nobles often held more extensive properties, and about 60 per cent of the estates lay east of the Elbe.

Table XIV

Time Differential	No. of cases
Award of title: same year as entail established or inherited	4
1 year before or after entail established or inherited	7
2 years " " " " " " "	7
3 years " " " " " " "	2
4 years " " " " " " "	1
5 years " " " " " " "	5
6-10 years " " " " " " "	6
11-20 years " " " " " " "	2
over 21 years " " " " " " "	1

⁴² Szögyényi to Kálnoky, Dec. 5, 1890, Preussen III, CXXXIX (*Berichte*), HHStA.

⁴³ Gerlach, *Erinnerungen*, 25.

⁴⁴ Johann-Heinrich Count Bernstorff, *Memoirs*, 64.

Table XV
Distribution of *Herrschaften* (All Creations)

Province	No. of <i>Herrschaften</i> held by nobles created, 1871-1918	No. of these for which hectarage is available	Total hectarage of these <i>Herrschaften</i>	Average hectarage of these <i>Herrschaften</i>
East Prussia	16	8	5,933	742
West Prussia	12	6	4,095	682
Posen	11	3	3,457	1,152
Pomerania	16	6	5,335	889
Silesia	32	13	12,666	974
Brandenburg	22	10	15,792	1,579
Saxony	17	7	7,308	1,044
Rhineland	17	7	2,881	412
Westphalia	4	3	1,402	467
Hanover	12	7	3,843	549
Hesse	2	1	460	460
Schleswig-Holstein	9	2	1,750	875
Unknown	21	1	300	300
Total	191	74	65,222	881

Examination of the Prussian nobility during the Empire points to certain conclusions and invites speculation on several issues.

There is evidence that the conferring of Prussian titles in this period was less a means for bringing along "new men" than for providing aristocratic shelter to marginal members of the older noble class. The effect, if not the purpose, of the creations was perhaps to intensify the division of classes rather than to blend them. In the first place, it appears that the creations who had been born bourgeois were well connected to the older aristocracy by birth or by marriage before or immediately following ennoblement, but deprived of noble insignia and status because of their father's bourgeois origins.⁴⁵ Of the creations from bourgeois ranks, 343 of 1,129 (slightly less than 31 per cent) had either an aristocratic mother or were married to noblewomen by the time of their creation or within six months thereafter. Some had both, and still others were wed to noble wives longer after their creation. A number who lacked noble mothers or wives could produce one or more aristocratic grandparents. In the second place, still other titles went to former bourgeois who were in some other way connected to the older nobility. Many *von*'s and barons, for example, were in fact recognitions of

⁴⁵ Ninety-eight per cent of all creations to the ranks of baron and higher had either aristocratic fathers, mothers, or wives, or were recognitions of foreign titles or confirmations of older Prussian titles.

Table XVI
Distribution of *Herrschaften* (All Creations)

	No. of <i>Herrschaften</i> created	No. for which hectare is known	Average size of such <i>Herrschaften</i>	Location of <i>Herrschaften</i>												
				EP	WP	Pos	Pom	Sil	Bbg	Sax	Rhld	Wfal	Hano	Hess	SH	Unk
D	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
P	2	—	—	—	1	—	—	—	—	—	—	—	—	—	1	—
C	6	3	1,210	—	1	—	—	1	—	—	—	—	1	—	2	—
B	20	2	303	2	—	—	1	2	1	2	3	1	1	—	1	6
v	163	69	884	14	10	11	15	29	21	15	14	2	10	2	5	15

foreign patents or confirmations of theretofore unauthenticated claims to ancient Prussian titles, or favors to illegitimate sons of nobles or to legitimate sons of life peers. In the third place, the groups that could be sharply differentiated by occupation from the nobles of more ancient lineage—in particular the professions, the world of business, the arts and sciences, and the naval service—are strikingly limited among the creations made during the Empire. Just under 65 per cent of all new creations or advancements went to men engaged in agriculture or military pursuits, both the traditional occupations of the Prussian aristocracy. And not a few of the professional men as well as almost all of the artists owed their elevations not necessarily to their professional distinction or success but rather to their intimacy with the crown as physicians in waiting, court painters, household bankers, and the like.

The new nobles drawn from the bourgeoisie, closely tied by kinship, origin, and occupation to the old Prussian aristocracy, were in their genealogical roots a remarkably homogenous group. This is also the case for those born noble and advanced in rank. No fewer than 435 of the 1,315 creations or advancements examined here went to men whose fathers and stepfathers, uncles, nephews, brothers and stepbrothers, or sons themselves received titles between 1871 and 1918 (see Tables II or X, pp. 762-63, 782-83). This takes into consideration only kinships between two connecting generations. If grandfathers, great-uncles, and so forth of those ennobled were also included, the percentages would be still larger. There were other associations of kinship between these nobles; it would be interesting to learn to what extent they married one another's brothers, sisters, or other relations.

Thus the nobles created by William I and his successors were a tightly constituted group with strong maternal, marriage, or occupational ties to the older aristocracy. A rigidity of structure characterized the entire nobility. In most cases the introduction of a bourgeois into the institution was apparently due to the fact that his occupation was either martial, agricultural, or bureaucratic, or that he was wealthy, and that he had come to the attention of the King. How far he progressed in the nobility—and how far those with noble fathers were advanced beyond the rank of their birth—apparently depended on who his mother was, whom he married, how much and what sort of land, or money, he possessed, the degree of distinction or of promise, or the eminence of rank he achieved in his profession as soldier or civil servant.

The absence of political figures in the new creations is another fact to emerge from this investigation of the nobility. Parliamentarians were virtually excluded. During the period covered, only four members of the *Reichstag* were ennobled while sitting in that body. It is interesting that three were representatives of conservative factions, and all were east Elbian landlords.⁴⁶ Another three were ele-

⁴⁶ The *Reichstag* members were: Ernst Flügge (*von*, 1886, Conservative, landlord in Pomerania); Ernst Staudy (*von*, 1888 [William II], Conservative, landlord in Posen); Georg Siemens (*von*, 1899, Progressive, banker-industrialist, landlord in Brandenburg); and Carl Gamp (*Freiherr*, 1907, Free Conservative, landlord in East Prussia).

vated before or after serving as deputies. Only thirteen members of the *Abgeordnetenhaus* and forty-seven representatives (many holding their seats by virtue of ownership of privileged landed estates) of the *Herrenhaus* were included among the new creations and advancements. Related to this point is the fact that the new nobility contained a relatively small number of bureaucrats. Those who had even an administrative association with the world of politics were overshadowed by the numerous agrarian and military figures who had also been elevated.

Titles awarded to military figures were almost exclusively at the *von* level and were frequently granted at the conclusion of active service. In order to determine patterns of title award and military rank, it would be necessary to compare carefully the dates of conferment of title with the date of rank and other information in the *Rangliste der königlich preussischen Armee*. This complex task lies beyond the scope of the investigation made here. Even a cursory examination of the *Ranglisten*, however, reveals several patterns in the ennoblement of military personnel. From 1871 to 1918 the ranks of colonel general and field marshal were restricted to men of noble birth, the only exception being General Anton Mackensen, awarded a *von* in 1899. Bourgeois promoted to the rank of full general were subsequently awarded titles unless they retired within one year of their advancement to general.⁴⁷ Many lieutenant and major generals were ennobled on active service; others were passed over. It would be interesting to know on what basis (military or other) the differentiation was made and what effect on an officer's subsequent career was produced by the award of a title. Did an ennobled major general, for example, win promotion to lieutenant general more quickly than a bourgeois? The same question would be of interest for officers granted titles at the rank of colonel or lower. It would also be enlightening to determine whether assignment to one or another section of the army (army corps staff, regimental command, quartermaster, and so on), to certain regiments, or to various types of service (cavalry, infantry, artillery, and so forth) affected an officer's chances for receiving a title. Finally, it appears that patents of nobility did not ensue immediately after one's promotion in military rank. The shortest time lag on record between promotion in military rank and award of title was five months. This was rare, however, and some generals waited for as much as ten years for their *von*. Two or three years seems to have been the usual period.

The prejudice detected against Roman Catholics in the naming of nobles is interesting. This phenomenon probably was due in part to the *Kulturkampf* since the number of Catholic elevations rose after 1879. Does the disinclination to elevate Catholics mean that Prussia's rulers were deliberately passing them over or that Catholics had never served in, or had retired from, or been rejected by, those occupations or social groups from which nobles were likely to be drawn? Was the percentage, or even the absolute number, of Catholic bureaucrats and military per-

⁴⁷ Minister of War Colonel General von Einem spoke of such elevations as "customary" (*üblich*). (Karl von Einem, *Erinnerungen eines Soldaten, 1853-1933* [Leipzig, 1933], 149.)

sonnel and landlords declining after 1871? Is the exclusion of German Catholics due to the fact that their numbers east of the Elbe were small? The Russian border provinces contained many Polish Catholics, but few were on good terms with their German overlords, and the Polish faction in the *Reichstag* was almost consistently opposed to the Conservative party representing *Junker* interests east of the Elbe.

Patterns of landownership also raise important questions. The pointed correlation between landowning and titles, between large, entailed estates and more exalted titles, the attachment to patents of conditions concerning land, the shadowy connection suggested by the figures on page 790 between the establishment of *Fideikomnisse* and the award of patents of nobility indicate the likelihood of a self-conscious policy on the part of the Prussian monarchy to encourage permanent settlement on the land, especially in the east. Did the crown deliberately use the granting of titles as an instrument to fortify the eastern, agrarian portion of the kingdom as a bulwark against the western, industrialized, more liberal parts?

Finally, the findings lead to questions about the monarchs themselves, particularly the last two. The only significant occupational differentiation between William I's nobles and those of his successors was that he preferred soldiers. But William was himself a soldier who had become Emperor through a victorious war, and his favors seem to have gone to solid men who had won the gratitude of their sovereign on the battlefield or to others who, like Bismarck, had made possible the successful merger of Prussia and Germany. It is curious, however, to find a man such as Frederick III, reputedly of liberal sentiments, showing a clear preference for agrarians among his creations. On the other hand, when contrasted with both his father and his son he showed a marked unwillingness to ennoble military figures. The continuing argument concerning the direction Prussia and Germany might have taken had he lived would be benefited by a more elaborate consideration of those whom he raised, as well as the reaction in various places to these grants.

In choosing his nobles, William II did not reveal himself as the "modern" ruler his handful of admirers claimed him to be. Most of his awards went to the same groups that his predecessors had favored and not to men in the forefront of industry, the arts and letters, and sciences, all areas in which conspicuous accomplishments were made during his reign. Rumors of venality, wholesale grants of titles, advancement of increasing numbers of favorites, and distribution of high ranks without the ownership of the extensive estates on which such titles traditionally had reposed undermined the standing of the sovereign. Some new or newly advanced nobles—Prince Eulenburg, Count Douglas, Adjutant Karl von Grumme, and others—expressed their gratitude to William II by a deference so exaggerated that it sometimes bordered on servility. This meretricious spectacle alienated many members of the older Prussian nobility from their King and inflated the self-esteem of a man whose vanity needed no replenishment.

“For God, for China and for Yale”—The Open Door in Action

JERRY ISRAEL

CIRCULATING his notes of 1899 and 1900, Secretary of State John Hay announced the goals of the Open Door: equality of commercial opportunity and maintenance of China's territorial and administrative integrity. It is impossible, however, to isolate one man or moment that gave practical meaning to Hay's words. The Open Door in action took shape in a combination of American enterprises, among which economic or religious activities were of outstanding importance. The individuals involved in these varied efforts—merchants, missionaries, or diplomats—voiced opinions not always in agreement on tactics. Yet, investigations of only one kind of American activity, economic or missionary, for example, fail to capture the interrelationship basic to a functional definition of American policy in China.¹

It is the purpose of this article to show several different components of the Open Door as neither strange nor infrequent partners. Economic institutions and reform organizations interacted with each other and together penetrated China. The people involved in them shared cultural conceptions, political philosophies, and bureaucratic backgrounds; they had common outlooks, goals, and destinies. In the combination of its parts, not in any single statement, tactic, or motive, lies an understanding of the Open Door in action.²

On the sixteenth of the Moon at the hour of *ssu*—September 14, 1905, from nine to eleven in the morning—Alice Roosevelt met the Empress Dowager of China. This real-life version of “the dream child moving through a land of wonders wild and new” carried for contemporaries a fascination similar to that of the fictional Alice's famous journey. In a vocabulary foreign to the usual State Department dispatch, the American Minister to China, W. W. Rockhill, reported to Secretary of State Elihu Root that “the department of the Imperial Household will

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¹ Two noteworthy single-perspective studies are Paul Varg, *Missionaries, Chinese and Diplomats: The American Protestant Missionary Movement in China, 1890-1952* (Princeton, N. J., 1958); and Thomas McCormick, *China Market: America's Quest for Informal Empire, 1893-1901* (Chicago, 1967).

² For suggestions along these lines from two different sources, see the review of Amaury de Rien-court, *The American Empire* (New York, 1968), *New York Times Book Review*, Sept. 22, 1968, 10; and the review of McCormick, *China Market*, *Journal of American History*, LIV (Mar. 1968), 910-12.

send a large chair with yellow loops to the Lang Jun Garden, in which Miss Roosevelt will be conveyed to the Jen Shou Throne Hall of the Summer Palace for Audience."³

The White Rabbit, the Mad Hatter, and the Queen of Hearts were not in attendance, but Theodore Roosevelt's daughter surrounded herself with an equally interesting cast of characters. One can perhaps ignore the American senators and congressmen and the crowd of reporters mostly concerned with the romance of the President's daughter and Nicholas Longworth. A close inspection of the passenger list, however, reveals that this extended Asiatic trip was more than just a social or romantic visit. Certainly the junket was planned to take advantage of the praise heaped on Roosevelt for his part in the recently completed Portsmouth negotiations to end the Russo-Japanese War. The President's desire to have his government officially represented, especially in possible discussions with the Japanese, explained the presence of Secretary of War William Howard Taft. But the appearance with Taft of important unofficial visitors, among them the railroad builder E. H. Harriman and Mabel T. Boardman, Clara Barton's successor as president of the reorganized American National Red Cross, hints that these adventures of Alice in the American wonderland of China were part of the first general stocktaking of United States diplomacy in Asia since the circulation of the Open Door notes and the coming to office of Roosevelt in 1901.⁴

Several changes in the summer of 1905 made such a rethinking necessary. Japan's rise to power and the death of Hay posed new challenges to the diplomacy of Roosevelt and of those like Taft who advised him on Far Eastern affairs. Taft's presence was, more directly, a response to the delicate balance that existed between the drive to the Open Door abroad and the fear of the yellow peril at home. Excited by insult and injury to Chinese visitors in the United States, Shanghai merchants were enforcing a general boycott of American goods, which Taft hoped to stop.⁵ This Chinese action threatened far-reaching repercussions. A trade boycott growing out of immigration and travel restrictions could conceivably lead to what the former Secretary of State John Foster called an "effective stop to all American enterprises in China." Bar Chinese students or upper-class tourists, Foster warned, and they will reciprocate by closing China to the whole network of interested Americans, be they "bankers, capitalists, railroad contractors, builders, and

³ On Alice's trip, see Rockhill to Root, Sept. 20, 1905, Department of State Dispatches, CXXVIII, National Archives [hereafter cited as NA]; Alice Roosevelt Longworth, *Crowded Hours: Reminiscences of Alice Roosevelt Longworth* (New York, 1933), 71, 73, 91-103; William Phillips, *Ventures in Diplomacy* (Boston, 1952), 23-24.

⁴ Roosevelt to Taft, Sept. 2, 4, 1905, Theodore Roosevelt Papers, Ser. 2, LVII, Manuscript Division, Library of Congress; Taft to Boardman, Sept. 16, 1905, Mabel Boardman Papers, *ibid.*; and Mabel Boardman, "An Audience with the Dowager Empress of China," *Outlook*, XC (Dec. 12, 1908), 824-28.

⁵ *Journal of the American Asiatic Association*, III (May 1903), 108; interesting secondary sources include Bruno Lasker, "Come in But Close the Door behind You: Chinese Exclusion in the United States," *Pacific Affairs*, XVI (Sept. 1943), 344-47; Dorothy Orchard, "China's Use of the Boycott as a Political Weapon," *Annals of the American Academy of Political and Social Science*, CLII (Nov. 1930), 252-61; and Y. C. Wang, "Free Enterprise in China: The Case of a Cigarette Concern," *Pacific Historical Review*, XXIX (Nov. 1960), 395.

engineers, mining experts and operatives, manufacturers and machinists, missionaries and physicians."⁶ A trade boycott might be the signal that the entire Open Door complement was to be locked out of China.

The presence of Harriman and Mabel Boardman in Alice's adventures gives reality to Foster's categories of interested Americans. Railroad builders like Harriman of the Union Pacific and the Great Northern's James J. Hill were interested in connecting their transcontinental railways to oceanic shipping lines and in building world-wide transportation systems. Hill, in fact, thought the Shanghai boycott portended "the greatest commercial disaster America has ever suffered."⁷ With cancellation of the American China Development Company's concession to build a Canton-Hankow railway threatening a similar disaster for American hopes of investment, Harriman's observations and negotiations took on an importance similar to Taft's.⁸ But neither man's discussions of America's commercial and financial future in China and Manchuria could be separated from the simultaneous involvements of Miss Boardman. In the midst of economic setbacks—indeed, some felt, the very cause of them—attempts at Chinese political, administrative, and educational reforms had not lived up to the promise held out for them in the Empire.⁹ As a representative of a major organization in the social welfare field, Mabel Boardman could see for herself and others what needed to be done.

In an economic sense, to the Harrimans and the Hills the Open Door in action was not an empty set of legalistic phrases about equality of opportunity and territorial integrity but a basic plan or system for the opening of China to American trade, investment, and railways.¹⁰ In a humanitarian sense (if that phrase is taken at its broadest), the Open Door in action represented to Mabel Boardman the last chance for China to stop its internal decay and to develop as it must under direct pressure from abroad.¹¹ The functional shape of what I have chosen to call the Open Door in action emerged from the way in which economics and humanitarianism complemented each other.

Much recent research has examined the insubstantiality of America's long dream in Asia.¹² In particular, Paul Varg, the author of an earlier study of

⁶ Foster wrote this comment for the *Atlantic Monthly*, XCVII (Jan. 1906), 127.

⁷ Hill is quoted in Charles Chaille-Long, "Why China Boycotts Us," *World Today*, X (Mar. 1906), 309-14.

⁸ On the American-China Development Company, see William Braisted, "The United States and the American China Development Company," *Far Eastern Quarterly*, XI (Feb. 1952), 147-65. For Harriman's interest, see George F. Kennan, *E. H. Harriman: A Biography* (Boston, 1922).

⁹ Contemporary statements of this are Paul Reinsch, "China against the World," *Forum*, XXX (Sept. 1900), 67-75; W. A. P. Martin, "China Transformed," *World's Work*, XII (Aug., Oct. 1906), 7844-48, 8115-25; and Wu Ting Fang, "Chinese and Western Civilization," *Harper's Monthly Magazine*, CVI (Jan. 1903), 190-92.

¹⁰ Such a view, of course, takes issue with the widely discussed interpretations of George F. Kennan as expressed in his chapters on United States' Asian policy in *American Diplomacy, 1900-1950* (New York, 1951).

¹¹ The argument associated with Miss Boardman was expressed by others such as the Open Door's chief devotee, Rockhill; see his "The Outlook in China," *Collier's*, XXVIII (Jan. 4, 1902), 9.

¹² Walter LaFeber, "America's Long Dream in Asia," *Nation*, CCV (Nov. 6, 1967), 456-59.

missionaries and diplomacy, has argued that "the rhetoric concerning the China market ran so wild as to suggest that [that market] was in the nature of a myth."¹³ Others have recognized the phenomenon, and, gauged by any statistical measure, Varg's thesis is well founded.¹⁴

This conclusion offers useful hints toward an understanding of the interrelationships and the common perspectives shared by the creators of the Open Door in action. The market was a myth because the basic institutions of Chinese society—its cities, education, transportation, communication, and government—made the country incapable of absorbing the exports of the American economy. To Varg, China's traditional society should have been evidence enough to induce pessimism and hopelessness among interested Americans. But to some of those interested Americans, the Harrimans and the Boardmans, "backwardness" was a challenge to be overcome. Success in modernizing the Celestial Kingdom would mean the end of the mythic quality of the dream.

Reform and progress, American-style, were not only necessary for their own sake but as the hinges on which the China door might swing open. In the aftermath of the Boxer rebellion and the Allied expedition to relieve Peking, Rockhill thought that there was no alternative for China but to "develop or decay." Reform, he wrote, must "come from without . . . under direct pressure from abroad."¹⁵ The interrelationship of business and reform, economics and ideology, was also noted by Seth Low, mayor of New York and an experienced China hand, when at a banquet of the American Asiatic Association in 1902, he toasted "prosperity, good government, peace and commerce."¹⁶

The "shared concepts" that Low toasted, notable in the activities of the Asiatic Association, a clearinghouse for China trade interests, were reflected in plans to use the indemnity for the Boxer rebellion to educate future Chinese leaders in the United States, in the idea of the diplomat John Barrett that there should be a China pavilion at the St. Louis Exposition in 1904, and in the establishment of the East Asiatic Committee working in conjunction with the American Museum of Natural History, the Metropolitan Museum of Art, and Columbia University.¹⁷ Headed by Morris Jessup of the Museum of Natural History and by the Colum-

¹³ Paul Varg, "The Myth of the China Market, 1890-1914," *American Historical Review*, LXXIII (Feb. 1968), 742-58.

¹⁴ The literature on the subject of dream and reality is large and growing; see the pathfinding Harold Isaacs, *Scratches on Our Minds: American Images of China and India* (New York, 1958). Robert Divine has described the mental attitude best when he writes, "Americans had developed a romantic view of China, visualizing it as a vast potential market for American goods, American culture and American democracy." (*The Illusion of Neutrality* [Chicago, 1962], 25.) Finally, the best single article on the subject is William L. Neumann, "Determinism, Destiny and Myth in the American Image of China," in *Issues and Conflicts—Studies in Twentieth Century American Diplomacy*, ed. George L. Anderson (Lawrence, Kan., 1959), 1-22.

¹⁵ See Rockhill, "Outlook in China," 9.

¹⁶ Low's toast appears in the *Journal of the American Asiatic Association*, II (Nov. 1902), 310.

¹⁷ An insight into the much-studied indemnity remission comes in George Marvin, "An Act of International Friendship," *Outlook*, XC (Nov. 14, 1908), 582-86, and "The American Spirit in Chinese Education," *ibid.* (Nov. 14, 28, 1908), 582-86; 667-72. On the exposition, see Minister Edwin Conger to Hay, July 3, 1903, Department of State Dispatches, CXXIII), NA.

bia University anthropologist, Franz Boas, this committee sought to found a great Oriental school in America in order to gain a deeper understanding of the peoples and countries of East Asia. Nowhere was the "sympathetic harmony" of leaders and intellectuals, institutions and interests more apparent than among the membership of the East Asiatic Committee. Working on behalf of what Boas described as "our commerce and political intercourse" was a roster reading much like a Who's Who of American economic interests in Asia: Hill and Harriman; Jacob Schiff, a partner in Kuhn, Loeb and Company; Clarence Cary of the American China Development Company; and John Foord, secretary of the Asiatic Association.¹⁸

The corporate activities of Standard Oil, Singer Sewing Machine, and other firms display a similar mixture of interests; so too do the institutional embodiments of the social gospel zeal of American missionaries, especially those centers of education and social work begun after the turn of the century. In this latter category special attention should be given to the Yale-in-China (or Yali) Medical College begun at Changsha in 1906 and the YMCA settlement house established as Princeton-in-Peking.¹⁹

Fired by the judgment of Secretary of State Hay that "the storm center of the world has gradually shifted to China," a group of Yale undergraduates met in New Haven in February 1901. They discussed what their role could be now that, as the Reverend Harlan Beach proclaimed, "dawn had broken on the hills of T'ang" and China had "opened her doors."²⁰ With what one member later recalled as "consummate self-assurance," Yale men led by two recent graduates, Warren Seabury and J. Lawrence Thurston, set out to "tell the world what was the matter with it."²¹

They had few plans, but they showed a pragmatic, flexible attitude troubling to more doctrinaire missionaries. By mid-1903 they concluded that only a new institution, a great Christian educational center, a veritable Yale-in-China, could win over the leaders of Chinese society. "Win the leaders," Thurston wrote to Beach, "and we win the Empire." As so often happened, reality, not the ideal of the Open Door, drove them to the remote Hunan Province to find a suitable and available home. From their school in Changsha, Henry W. Luce, father of the well-known publisher, wrote home to New Haven that in Hunan they had "a

¹⁸ *Journal of the American Asiatic Association*, III (Feb., May 1903), 7, 108, 110.

¹⁹ Much of the following discussion results from research in seldom used papers. The Sterling Memorial Library of Yale University houses the archives of Yale-in-China, which, under the name the New Asia College, is still functioning in Hong Kong. There is also an officially sponsored study by Reuben Holden, *Yale-in-China: The Mainland, 1901-1951* (New Haven, Conn., 1964). In addition, the files of the YMCA World Service may be consulted at the organization's national headquarters' library in New York. There is also much material on such activities at the Union Theological Seminary's Missionary Research Library in New York.

²⁰ See Harlan P. Beach, *Dawn on the Hills of T'ang: Or Missions in China* (New York, 1899); Reginald Wheeler, *Flight to Cathay* (New Haven, Conn., 1949); and Anson Phelps Stokes, *A Visit to Yale-in-China, June 1920* (New Haven, Conn., 1920).

²¹ The recollections of A. B. Williams in 1944 are reported in Holden, *Yale-in-China*, 9, 77.

clear field to work out a consistent and harmonious plan from the lower schools to the higher." As a consequence they could exert "a wide influence on a large body of people. We may work together," he sincerely hoped, "for God, for China and for Yale."²² By 1906, the institution at Changsha, which would eventually include a school of arts and sciences and a preparatory school in addition to a medical college, was functioning as a center for the "uplifting of leading Chinese young men toward civilization." In this way, though not so rapidly or significantly as Luce may have dreamed, Yale and America could establish a "stable and progressive government" in China.

Other educational advisers were dispatched, more American university clubs were established, and existing Christian colleges were strengthened. As is demonstrated by the work of Yale-in-China and of rival groups founded at such institutions as Wisconsin, Princeton, and Brown, education was not simply an outlet for missionary fervor but a means to shape a new civilization in the Far East, one that would be as Christian and professional as American society. "Western education in China is only one side of a movement," a graduate study committee at Peking University reported. The task "is not simply introducing new ideas into the country but modifying its industrial, social and political life and institutions."²³

Another American organization joining in this drive to remake China was the YMCA. Implored by John R. Mott of the Student Volunteer Movement for Foreign Missions not to be "satisfied with exerting influence on one side of the world," Fletcher Brockman and D. Willard Lyon had sailed for China in the mid-1890's. The Association, as it came to be known in China, set itself up in 1899 in the great cities, particularly in Shanghai, as an organization for businessmen residing in the port. It soon came to be China's first Western-style social work group. To Yale's American-style classroom, the YMCA added social settlement houses like those of Lillian Wald on Henry Street in New York and Jane Addams on Halsted Street in Chicago.

The Association devoted most of its work to correcting urban woes much like those it sought to eradicate in America, such as gambling and prostitution. Feeling a need to bring Jesus of Nazareth to the "open door in Paotingfu" as well as a desire to be part of the reorganization of China's national systems of finance, defense, and education, the leadership of the YMCA in China sponsored industrial education through lectures. At Robert Lewis' Shanghai headquarters, or the Association's Princeton-in-Peking office, members of China's educated upper class

²² Thurston to Beach, June 8, 1903, Thurston-Seabury Correspondence, 1903-1906 file, Yale-in-China Archives; Luce to Professor E. B. Reed, May 10, 1904, Henry Luce file, *ibid.*; see also a 1906 progress report in the Thurston-Seabury Correspondence, *ibid.*

²³ John Barrow, "American Institutions of Higher Learning in China, 1845-1925," *Higher Education*, IV (Feb. 1, 1948), 121-24. The best study of such activities appears in Urban G. Whitaker, Jr., "Americans and Chinese Political Problems, 1912-1923," doctoral dissertation, University of Washington, 1954, Chap. IV. An excellent review of the goals of another university, Wisconsin-in-China, appears in the Charles R. Crane Papers, Appendix No. 2, dated Jan. 10, 1922, held by Crane's son, John O. Crane, Institute for Current World Affairs, New York. A Brown-in-China program is discussed in the John Freeman file, Dec. 17, 1920, in the Yale-in-China Archives.

were told about the steam engine, the railway, and the electric telegraph. The YMCA sought to teach the "languages, commercial customs and business methods of the west."²⁴

In search of useful lessons from home to apply in China, the YMCA hit upon the idea of the boys' camps popularized by Daniel Carter Beard and the Boy Scouts. Such groups, a spokesman for the YMCA thought, "can become as great, if not a greater influence for good in China than in America."²⁵ Although the Association never forgot that even in China the New Testament "is a book which we must all read," many of its efforts were directed toward sanitary improvement of China's prisons, parks, and places of business. The accent of such social uplift was always on the "new" or "young" or "awakened" China, for the "children of today will become the rulers and leaders of tomorrow, and they must be nurtured and raised with the greatest care."²⁶ Chinese "jinrikishamen" were to be turned into athletes for future Olympic games. If qualified physical directors could be imported, the Association would "direct the policy and control the future of the athletic situation among four hundred millions of people." Using familiar American imagery, J. H. Crocker looked ahead to China's future achievements in sports: "The door," he said, "is wide open in all large cities." Young Chinese, "with their fine physiques and Occidental speech and manners" could be trained in the classroom, settlement house and athletic field to become "the most progressive element in the country."²⁷

The great commission of the YMCA was indeed that of all independent American reformers in China: to wash, feed, clothe, educate, and employ an awakened people. D. Willard Lyon, an original Association China hand, was confident that America was uniquely fitted to guide such a process of maturation. "Having had experience in developing the resources of an expansive territory and having produced the machinery and methods necessary for carrying on large enterprises," Lyon observed, "America is in a peculiarly favorable position to supply what China needs."²⁸

Perhaps no enterprise illustrates the interaction that shaped the Open Door policy in operation as well as the attempt of Alice Roosevelt's friend, Mabel Boardman, to bring flood relief and river conservation to China. That effort explains

²⁴ D. Willard Lyon, *The First Quarter Century of the YMCA in China, 1895-1920* (Shanghai, 1920), found in the Missionary Research Library; see also Galen Merriam Fisher, *Public Affairs and the YMCA, 1844-1944* (New York, 1948), 161-64. On the origins of the work in China, see Mott to Brockman, Jan. 3, 1899, YMCA World Service Files, x951, folder D, 1898-1900, and Lyon to Mott, May 24, 1900, *ibid.*

²⁵ See the note of J. C. Clark, Aug. 5, 1913, YMCA World Service Files, folder C, 1912-14.

²⁶ George Lerrigo to F. M. Mohler, May 23, 1913, Lerrigo Correspondence, *ibid.* These YMCA papers are rich in field reports; see, for example, J. S. Burgess, "The Training of Social Workers in China," *ibid.*, x951, 101, and "Peking as a Field for Social Service," *ibid.*, folder C. Consult also "Ideals and Activities," a report by the Chinese Students' Christian Association of North America, May 1913, in the John R. Mott Papers, box 690, Yale Divinity School Library. Unfortunately the Mott Papers are still largely closed pending an official biography.

²⁷ J. S. Burgess, "China's Social Challenge," *Survey*, XXXIX (Oct. 13, 1917), 41-44; J. H. Crocker, "Annual Report," Sept. 30, 1914, YMCA World Service Files.

²⁸ D. Willard Lyon, "Annual Report," Sept. 10, 1908, *ibid.*

how appropriate it was that she should travel with Taft and Harriman in 1905. Taft, for example, returned to Asia two years later to extol the virtues of equal doses of administrative reform, resource development, and greater efficiency so as to improve the general welfare and create a truly open door.²⁹ Harriman, too, was deeply concerned in bringing American bankers, railroaders, engineers, consuls, diplomats, and publicists together in an integrated effort to enhance investment plans in Manchuria and to forge his world-wide transportation network.³⁰

Miss Boardman's intention was to improve the capacity of China's rivers to receive ocean-going vessels bringing trade and relief, oil and food. River reclamation would improve trade, increase investment opportunities, and further soil conservation. One specific plan that received attention throughout the decade following Miss Boardman's visit was a scheme to create outlets from the Hwai River Basin to the sea, thus eliminating floods and creating new farmland.³¹ Paul Reinsch, American minister to China in the Wilson administration, observed that this half-commercial, half-humanitarian work "could impress the Chinese mind so vividly with the true meaning of the word PROGRESS." He did not overlook the practical result of developing China's resources, "for while railways create means of communication, this enterprise will create the most fundamental of the constituent parts of wealth and welfare—agricultural lands of steady productivity over an area nearly as large as the state of Maine."³²

The Standard Oil Company was active in financing such work as this with loans secured by oil monopoly rights. The Taft and Wilson administrations agreed that the Hwai project would be "singular" evidence of serving a "great nation just awakening."³³ With this support, Standard Oil's confidence and money came to be matched by the even greater resources of American banks.³⁴

It was characteristic that the spark needed to propose, plan, and implement the Hwai project came not from a commercial or financial organization but from the Red Cross. Such a multifaceted project as a philanthropic and business effort at flood relief that was also an experiment in scientific conservation was central to the purpose of Miss Boardman's restyled Red Cross. The principle of

²⁹ New York Times, Oct. 9, 1907; *Journal of the American Asiatic Association*, VII (Dec. 1907), 326.

³⁰ See Richard T. Chang, "The Failure of the Katsura-Harriman Agreement," *Journal of Asian Studies*, XXI (Nov. 1961), 65-76. An interesting view of the Harriman plan as written by a contemporary is George Kennan's biographical article, "E. H. Harriman," *Asia*, XVII (June 1917), 271-76.

³¹ Most of the following discussion is derived from the extremely valuable American National Red Cross file 898.5/2, Hwai River Conservancy, American National Red Cross Headquarters' Library [hereafter cited as RCHL].

³² See Reinsch to Secretary of State William Jennings Bryan, Oct. 28, 1914, *ibid.*; Reinsch to Boardman, Nov. 27, 1914, *ibid.*

³³ Secretary of State Philander Knox to Henry P. Fletcher, Nov. 2, 1909, Henry Fletcher Papers, Manuscript Division, Library of Congress; Assistant Secretary of State E. T. Williams, press conference, Oct. 18, 1913, Paul Reinsch Papers, Wisconsin State Historical Society.

³⁴ Wilson to Senator George Chamberlain, Apr. 7, 1914, Woodrow Wilson Papers, Ser. 4, Case File 227, Manuscript Division, Library of Congress; *Journal of the American Asiatic Association*, XIV (Mar. 1914), 33-34, 39.

her organization would be "honest and efficient" work designed to create "economy and social advantage."³⁵

Despite the presence on the Red Cross International Relief Board of former Secretary of State Root, the international lawyer John Bassett Moore, or the young Assistant Secretary of the Navy Franklin Delano Roosevelt, it was Mabel Boardman herself who made "the necessary financial arrangements with American financial and industrial interests."³⁶ Charged by sections of the press with allowing the Red Cross to sacrifice philanthropy by pulling American "dollar diplomacy chestnuts out of the fire," Miss Boardman admitted that "we are not a business organization." It remained her "serious duty," however, to ensure that "American bankers and contractors may be willing to cooperate with the American Red Cross in this work for China."³⁷

Through the urgings of her close friend Taft, Miss Boardman had met Henry Davison, a partner in the banking house of J. P. Morgan. In 1914 Davison became the chairman of the governing board of the Red Cross, and Miss Boardman set about her "serious duty." "Using several other loan agreements approved and accepted by the Chinese government," Miss Boardman noted, "I drafted one as a basis for decision. I then submitted it to the Chinese minister . . . and to several prominent bankers who are personal friends and with whom I have already had brief conferences." The result was a proposed twenty-million-dollar loan, with revenues from the improvement, sale, and lease of all reclaimed land as security, to be financed through the National City Bank of New York, the International Banking Corporation, and the newly formed American International Corporation.³⁸

The constant presence on the Hwai project of a chief engineer named C. D. Jameson offers a revealing side light. As principal adviser on the technical side of the task, Jameson harbored few illusions about educational or political reform; it was typical of him to refer to a Chinese graduate of an American college as a "hopeless, half-baked B. A. (Bally Ass)."³⁹ Despite his manifestly unpleasant disposition, Jameson was sought after and favored by both the Red Cross and the American State Department, and, when not slandering the Chinese, he expressed the spirit and purpose of the Hwai project and the Open Door in action.⁴⁰ "Conservancy will do more towards opening up the country, alleviating suffering

³⁵ Boardman's speech, 1910, before the National Conservation Congress, St. Paul, Minn., Boardman Papers; also Boardman to John D. Rockefeller, Jr., Apr. 25, 1913, Red Cross file 898.5/2, RCHL.

³⁶ Boardman to Robert Lansing, Mar. 26, 1914, Robert Lansing Papers, Manuscript Division, Library of Congress; Boardman to Wilson, July 25, 1913, Red Cross file 898.5/08, RCHL.

³⁷ Boston *Evening Transcript*, Apr. 16, 1914, found in Red Cross file 898.5/7, "Publicity," *ibid.*; Boardman to Robert W. deForest, Jan. 20, 1914, Red Cross file 898.5, *ibid.*; and Boardman to Wilson, June 11, 1914, Department of State Decimal File 893.811/152, NA.

³⁸ Red Cross file 898.5/2 "Loan Agreements," RCHL, contains the key document of Boardman to Reinsch, June 15, 1914, explaining her role; see also Moore to Boardman, Sept. 8, 1914, *ibid.*

³⁹ Much material on Jameson can be found in Red Cross file 898.5/08, "C. D. Jameson," *ibid.*

⁴⁰ Jameson to Boardman, Aug. 20, 1912, Department of State Decimal File 893.811/75, NA, and Reinsch to Bryan, Dec. 19, 1913, Department of State Decimal File 893.811/98, *ibid.*, indicate desires to have Jameson on the Hwai project.

and conducing to quietness and productiveness among the people," Jameson insisted, "than any other proposition in existence." There was a moral necessity to end the degeneration of millions "who are now becoming beggars and robbers"; these same millions would then be converted from "Non-producers" to "Producers." Last but not least, the river project was "justifiable from a financial point of view," throwing open the doors to new investment groups.⁴¹

Reaction to the Hwai project as carried out by the Red Cross reflected its linked commercial and humanitarian purposes. Charles W. Eliot of Harvard thought that it "crowned unselfish American enterprise in China for the betterment of human life and true progress." Reinsch, agreeing that the whole enterprise was representative of American-Chinese relations, was sure that the United States must "reap the reward which always comes from taking the lead in great improvements of this kind." Merchants, missionaries, and industrialists would all benefit from what Taft saw in the Hwai project: "economy and efficiency" signaling the "dawn of a scientific and international new era."⁴²

For all this glowing praise, it must be noted that the Hwai project, like so many other Open Door activities, improved few human lives, reaped no great rewards, and opened no new eras. Competition from more urgent Red Cross relief needs in Europe during and after World War I provided a realistic excuse for the plan's lack of success.⁴³ In its failure, the Hwai conservancy underlines the argument about the "mythic" quality of Open Door plans. It is quite as important, however, in showing that such dreams of success rested on the interaction of many American individuals and interests.

There were few areas of activity in which Americans were not convinced that they could remake China in the image of the United States, as "a self-reliant, progressive power." Efforts, labeled "progressive and humanitarian," were inaugurated to abolish slavery, to develop telegraph and customs services, to modify the existing penal code by eliminating "slicing, exposure of the head, beheading of the corpse, strangulation and branding," to build an army and navy, and, symbolically, to outlaw the ancient female tradition of foot-binding.⁴⁴ While a suffragette movement was gaining support in the United States, that door also opened in the Far East. The duty of a Chinese woman was to be married in a polygamous society where concubinage was common. One in ten thousand Chinese women could read and write. Two hundred thousand girls a year did not survive infancy. The National American Women's Suffrage Association and the Women's Boards

⁴¹ Jameson to General George Davis, Mar. 21, 1913, Red Cross file 898.5/2, RCHL; Jameson preliminary report of 1913, 43, Red Cross file 898.5/08, *ibid*.

⁴² Eliot to Crane, Feb. 7, 1914, Crane Papers; Reinsch to Wilson, Nov. 28, 1914, Wilson Papers, Ser. 4, Case File 1953; and Taft's speech, "The Influence of the Red Cross for Peace," delivered at Clark University in 1915, Boardman Papers.

⁴³ On the project's failure, see Crane to Wilson, Sept. 22, 1920, Crane Papers; Taft to Boardman, Nov. 28, 1920, Boardman Papers.

⁴⁴ That the State Department was interested in such informal, reform diplomacy is shown in the correspondence of Rockhill to Root, July 19, 1905, Apr. 24, 1906, State Department Dispatches, CXXVII, CXXX, NA, John Gardner Coolidge to Hay, Apr. 26, 1905, CXXVII, *ibid*.

of Foreign Missions felt sure that "all workers in reforms realize" that "the advance of civilization is relative to the status of women."⁴⁵ While suffragettes pressed to give women the vote in Shanghai and Peking, other reformers were equally adamant about the necessity to build up a primitive postal service, to introduce the typewriter or the wireless, and even to bar liquor from the legation in Peking.⁴⁶

An enthusiastic belief that China could and must be reformed overcame distress at the backward condition of contemporary institutions. Neither the myth nor the irony of an unrealized market, so clear to historians such as Varg, was visible to Americans in China. Rather, the very hope that conditions would be improved by their presence explains the blindness to the gap between dream and reality and provided the common assumption behind both the economic and religious channels of American expansion.

The image of China as a *tabula rasa* for American reform as well as for trade and investment linked Yale-in-China, the YMCA, and the Red Cross to the main thrust of American diplomacy in Asia. Consuls general, trade attachés, and missionary leaders all pleaded for the United States to export the kinds of things being made and being done so well at home. Systematic educational welfare programs and the introduction of administrative and engineering methods could not but produce progressive Chinese and a matchless opportunity for the development of American business enterprises.⁴⁷ Returning from a visit to the Far East, William Jennings Bryan announced his belief "that no Christian nation can justify doing business" unless its citizens "interest themselves in the people among whom they go." To this end, on New Year's Day of 1912 Bryan sent an encyclopedia of Thomas Jefferson's views to China's first post-Manchu leader, Yüan Shih-k'ai, and greeted him with the hope that this "awakening" might produce a "United States of China."⁴⁸

Outside observers, like the British philosopher Bertrand Russell, envisioned grave dangers in such an American "invasion." From Peking in 1922, Russell wrote: "if America is victorious in the Far East, China will be Americanized and

⁴⁵ Kate Gordon to Conger, Oct. 14, 1901, CXV, *ibid.*; Conger to Hay, Dec. 4, 1901, *ibid.*; Mrs. John R. Mott's views can be found in *Evangel*, II (May 1899), 6-9, and in the Mott Papers, FA 546; see also Lorsa Maxon Holmes to Wald, Jan. 4, 1900, Lillian Wald Papers, file case 1, drawer 1, New York Public Library.

⁴⁶ See "The Mails of the Mandarin," *World's Work*, XXXIII (Apr. 1917), 637-44; *National Advocate*, LVI (June 1921), on prohibition, found in State Department Decimal File 893.114/298, NA; Bryan to Reinsch, June 26, 1914, on the wireless, in State Department Decimal File 893.74, *ibid.* Mrs. Mildred Crow made it her business to sell typewriters. (*World Outlook* [Aug. 1916] in the William Elliott Griffis Papers, Rutgers University Library.)

⁴⁷ See a statement by Consul Thomas Sammons, Oct. 10, 1919, State Department Decimal File 693.001/151, NA. Merle Curti, *American Philanthropy Abroad: A History* (New Brunswick, N. J., 1963), 348-53, fails to understand the economics of reform, but is still one of the few broad studies of America's parental role in China. For a contemporary statement, see the *New Republic*, XIX (May 10, 1919), 45-47.

⁴⁸ Bryan to the World Missionary Conference, 1910, in World Missionary Conference, *Reports of Commissions III, VII and VIII* (New York, 1910); see also Bryan to Yüan, Jan. 1, 1912, William Jennings Bryan Papers, box 28, Manuscript Division, Library of Congress.

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though the shell of political freedom may remain, there will be an economic and cultural bondage beneath it."⁴⁹ Yet to those most involved in the "Americanization," unpredictable as may have been the consequences of their actions, "bondage" was hardly the goal. Writing to John D. Rockefeller, Jr., Mabel Boardman remarked about the Hwai project that "It would be easier for us to offer to assist," since everyone would be "more ready to believe that an engineer sent by the American Red Cross has no other motive than an altruistic one."⁵⁰ This statement, by way of conclusion, emphasizes the interrelationship characteristic of American influence and policy in Asia. Missionaries and businessmen, Red Cross relief workers, and Wall Street bankers brought differing perceptions, goals, tactics, and conflicts to the Hwai project and to any Open Door activity. Yet the eventual shape of that activity emerged not from any single motive, purpose, or agent, but from the mutual energies and combined personalities of all involved.

Attention to only one side of the Open Door might easily color Miss Boardman's statement to Rockefeller about Red Cross participation with either of two highly charged yet conflicting interpretations. If one sees altruism, reform, or ideology as rationalization and camouflage, then Miss Boardman had rather baldly admitted the conscious duplicity of the Red Cross in the American attempt to penetrate China. If, on the other hand, religious, humanitarian, or ideological motives transcend more material matters in one's scheme of things, then Miss Boardman had quite simply defined a pragmatic necessity for Red Cross responsibility to China. Few historians, however committed to either interpretation, deny the existence of other factors. Yet, the crucial task is not to recognize reluctantly a set of separate forces, but to see them as they complemented each other. Ocean-going vessels would bring both relief and oil, Red Cross engineers and Standard Oil salesmen. It was as unusual to find the oil company's agents without philanthropic and scientific support as to discover a Red Cross famine relief team without commercial and financial backing.

The Open Door in action was neither a heartless, calculated exploitation, nor a foolish, naïve crusade. From its earliest foundations American foreign policy functioned to preserve, protect, and expand a progressive, business, and Christian society. Reforming cultures, making profits, and saving souls were not incompatible goals, or so it was felt. Rather, each activity worked, sometimes consciously, sometimes not, to the other's advantage. Succeed, as it was hoped they would, and together they would grow. Fail, as it was seldom feared they would, and together they would fall.

⁴⁹ Bertrand Russell. *The Problem of China* (London, 1922), 147.

⁵⁰ Boardman to Rockefeller, June 5, 1911, Red Cross file 898.5/2, RCHL; Boardman to Starr J. Murphy, June 30, 1911, *ibid*.

* * * *Review Article* * * *

The Historical Writing of Jaime Vicens Vives

GABRIEL JACKSON

AN ECONOMIC HISTORY OF SPAIN. By *Jaime Vicens Vives*. With the collaboration of *Jorge Nadal Oller*. Translated by *Frances M. López-Morillas*. (Princeton, N. J.: Princeton University Press, 1969. Pp. viii, 825. \$13.50.)

THE appearance of an English translation of the *Historia económica de España* provides a welcome opportunity to review the magnificent contribution to historical literature made by the late Jaime Vicens Vives (1910-1960). Vicens was a man of extraordinary energy. Besides producing a dozen books and several dozen articles, he established a publishing house, founded several periodicals and institutes, trained and inspired numerous graduate students, and organized uniquely effective team research in statistics and demography.¹ In the present essay, however, I shall not deal with his career as a teacher and entrepreneur, but shall try to assess the importance of his individual writings in three distinct areas: fifteenth-century Aragon-Catalonia, nineteenth-century Spain and Catalonia, and the general economic and social history of Spain.

Vicens' earliest and most detailed archival research concerned the kingdom of Aragon, which, in the second half of the fifteenth century, consisted of three different sets of territories, united only by the accidents of dynastic history and maritime enterprise. There were the sparsely inhabited agricultural and pastoral provinces of the Pyrenees, the agriculturally and commercially booming provinces of Catalonia and Valencia, and the insecurely held Mediterranean islands of Sicily, Sardinia, Corsica, and the Balearics. The peninsular territories lay between France and Castile and were constantly coveted by these more populous and militarily powerful neighbors. The islands were coveted by France, Genoa, the papacy, and the Muslim pirates. Barcelona was one of the most prosperous Mediterranean ports, but the Catalan countryside was undergoing a social revolution which eventually freed a large proportion of its peasants, the *payeses de remensa*, from an onerous feudal regime.

► Mr. Jackson, a professor at the University of California, San Diego, received his Ph.D. degree from the University of Toulouse in 1952. Chiefly interested in modern European history, he is the author of *The Spanish Republic and the Civil War* (Princeton, N. J., 1965).

¹ For an outline of Vicens' life, including his wide-ranging activities as a teacher, organizer, and publisher, and a full list of his writings, see Stanley G. Payne, "Jaime Vicens Vives and the Writing of Spanish History," *Journal of Modern History*, XXXIV (June 1962), 119-34.

In *Juan II de Aragón* (Barcelona, 1953), *Fernando el Católico, Príncipe de Aragón, Rey de Sicilia* (Madrid, 1952), and *Historia crítica de la vida y reinado de Fernando II de Aragón* (Saragossa, 1961) Vicens concerned himself primarily with the dynastic and imperial aspects of the half century. In *Historia de los remensas en el siglo XV* (Barcelona, 1945) and *El gran sindicato remensa* (Barcelona, 1954) he dealt primarily with the social revolution. Both his personal research and his wider interpretation are compactly summarized in *El siglo XV, Els Trastàmars* (Barcelona, 1956).

All these works are characterized by a wealth of archival references, a vigorous, frequently polemical style, a human concern both with leading actors and with the common man, close attention to economic interests, family and class rivalries, a forthright Catalan patriotism, and a critical attitude toward earlier historians of the era. Certain themes reappear constantly. In relation to peninsular politics as a whole the members of the Aragonese branch of the Trastamara family are seen as allies of the *converso* bourgeoisie. The successive kings were all equally ambitious to develop their family wealth and the national economy. Ferdinand of Antequera (1412-1416) amassed large estates for his children. Alfonso V, "The Magnanimous" (1416-1458), concentrated on the development of the Mediterranean islands. Juan II (1458-1479) allied himself with the more progressive peasantry in the internal conflicts of both Navarre and Catalonia, maintained his links with the *converso* economic leadership in Castile, and never wavered from his plan to unite Castile and Aragon under the Trastamara family. His dynastic ambitions triumphed with the marriage of his son Ferdinand to the latter's cousin Isabella in 1469. During and after the Catalan civil war of 1462-1472, and throughout the conflict over the money payment (*remensa*), both Juan II and Ferdinand the Catholic (1479-1516) allied themselves with the prosperous peasants and bourgeoisie of Catalonia.

While he deals in some measure with the entire range of fifteenth-century Hispanic history, Vicens focuses most heavily on the dynastic ambitions and problems of Juan II and on the *remensa* conflict. Juan II had two sons: Carlos, prince of Viana, whose mother was Queen Blanche of Navarre, and who was therefore heir to the thrones of both Navarre and Aragon; and Ferdinand, whose mother was Juana Enríquez, daughter of the powerful and wealthy Fadrique Enríquez, *almirante* of Castile. Ferdinand was always the favored son, first on grounds of his maternal family connections, and later through his father's recognition of the younger son's greater political ability.

In 1460 the family conflict suddenly became intertwined with the politics of the agricultural revolution in Catalonia. The objective of *remensa* agitation ever since the late fourteenth century had been to free the Catalan peasants of the money payment, which was required of them by their hereditary landlords. Originally the *remensa* was the price paid for permission to leave the land. It had been de-

signed to slow down the emigration of peasants to the city. By the mid-fifteenth century it was only the most hated of numerous taxes (*los malos usos*) being collected more vigorously than in earlier decades by landlords who were feeling the pinch of rising prices and labor shortages attendant upon the expansion of commerce and industry in Barcelona. At the same time the urban patriciate of Barcelona, self-appointed defenders of the constitutional liberties of Catalonia, were among the powerful landlords and investors whose income depended in part on the collection of the *remensa* and other feudal taxes. The patricians had long resented the tendency of both Alfonso V and Juan II to favor the abolition of the *remensa*. They suspected the Kings of supporting the peasantry in order to prevent the growth of a strong Catalan autonomist movement under patrician leadership, and they insisted that the direct intervention of the Aragonese sovereigns in the *remensa* conflict was a breach of the constitutional liberties of Catalonia.

Now it was customary, though not legally binding, for the king to name his eldest son as his "lieutenant" in Catalonia. In 1460, when Juan II tried to pass over his elder son Carlos in favor of the eight-year-old Ferdinand, the province rallied, under patriotic and constitutional banners, to the cause of the Prince of Viana. The unfortunate Prince died in 1461, and the King's effort to install Juana Enríquez, Ferdinand's mother, as governor, precipitated a decade of revolution and civil war. The older urban patriciate, supported by a large fraction of the Barcelona artisans and small tradesmen, led the revolt against Juan; the majority of the prosperous peasants supported him; the nobility and the clergy were divided. Dynastic and cultural loyalties clashed with economic interests. Thus some landlords and their *remensa* enemies sided with the Aragonese monarch, while other landlords and poorer peasants opted for an independent Catalonia. Some merchants and artisans chose Catalan nationalism over their pragmatic interests; others favored Juan II on grounds of the national unity of Aragon-Catalonia and the intelligent economic policies of the King.

The Spanish historian Eduardo Hinojosa had dealt with the *remensa* conflict, but had interpreted the liberation of the peasants as part of a uniform tendency, from the mid-fourteenth century onward, for the condition of the peasantry to improve.² Vicens shows clearly that a species of feudal reaction in the fifteenth century had increased the tax burdens of the Catalan peasants. The French historian Joseph Calmette had dealt with the international ramifications of the struggle between Juan II and the Barcelona oligarchy, but he had barely mentioned the internal factions and class conflicts.³ Vicens claims to have superseded Calmette, but actually the works of the two men complement each other. The

² Eduardo Hinojosa, *La pagesía de remensa en Cataluña* (Barcelona, 1902), published in French translation as *Le servage en Catalogne* (Paris, 1902), and *El régimen señorial y la cuestión agraria en Cataluña* (Madrid, 1905).

³ Joseph Calmette, *Louis XI, Jean II, et la révolution catalane* (Paris, 1902), and *Le grand règne de Louis XI* (Paris, 1938).

later historian emphasized the economic and social aspects of the civil war. His predecessor had written excellent dynastic and diplomatic history, and indeed if the reader simply bears in mind that the persons whom Calmette treats as representing Catalonia represent only the dominant patrician-artisan alliance, the French historian's work remains superior in terms of clear narrative and overall international perspective. Vicens' vigorous style sometimes obscures the fact that his own detailed documentation shows the impossibility of making clear generalizations about class alignments during the Catalan revolt.

The second major area of Vicens' personal research and writing was the nineteenth century. While he was interested especially in the industrial and urban development of Catalonia, he always treated Spain as a unit; it is therefore appropriate to discuss simultaneously the nineteenth-century chapters of *An Economic History of Spain*,⁴ his personal chapters in Volume V of the *Historia social y económica de España y América*, and *Els Catalans en el segle XIX*.⁵ In all this writing Vicens makes extensive use of the available statistics regarding demography; industrial, mineral, and agricultural production; property transfers, tax, tariff, and banking receipts. He describes in significant detail the development of industrial techniques, of business and labor organizations, and the effects of tariff policies, railroad construction, emigration, and foreign markets for Spanish exports. Prefacing his generalizations with a warning that historians really know very little concerning the inner dynamic of the nineteenth century, he suggests three main stages for the years 1808–1898. The period 1808–1843 was one of overall depression and institutional uncertainty, opened by the Napoleonic invasion, and including abortive revolutions, the loss of the main American colonies, and the Carlist War. In the second stage, 1843–1868, Spain equipped itself industrially, enjoyed the stimulus of foreign investment, and brought much new land under cultivation. But the economic benefits did not reach the peasants and industrial workers, and a much too restricted minority of the population could take part in the political process. After the revolutionary (and free trade) interval of 1868–1874, the Restoration era (1875–1898) witnessed accelerated economic development, and the state, which had previously thought in terms of purging one group or another, now took a neutral attitude toward Spaniards of all political and intellectual persuasions.

While these outlines are highly suggestive, there are many inconsistencies between Vicens' data and his interpretations, which must inevitably make the reader question even these tentative formulas. In discussing the Restoration, he refers to a "great leap forward" (p. 615) in the agrarian economy. The statement is partially validated by his statistics on vine production and olive oil and citrus exports (pp. 646, 653). But wheat and cereal production per inhabitant declined between 1860

⁴ Parenthetical page references in the text are all to the English-language edition.

⁵ Jaime Vicens Vives, *Historia social y económica de España y América* (5 vols., Barcelona, 1957–59), and *Els Catalans en el segle XIX* (Barcelona, 1958).

and 1900, with yields per hectare rising only slightly in relation to the base year 1800. Sheep were about 20 per cent more numerous in 1910 than in 1803, but the numbers of cattle and pigs were substantially the same in both years, whereas the human population had increased by about 80 per cent. These facts would suggest that the ordinary Spaniard may not have been eating as well in 1900 as in 1800. In any case the "great leap forward" applied to a very limited sector of the agrarian economy.

Patriotic motives, and the consciousness of different audiences, probably explain the inconsistency of his several discussions of the cork industry. In the *Historia social y económica*⁶ and, in virtually the same words, in *An Economic History of Spain* (p. 677) he vaunts the superior quality of Catalan corks, Catalan influence on cork forest care and financial prosperity in other provinces, export miracles, and the unique character of the Catalan cork industry: "optimistic, gay, democratic, without class distinctions." In *Els Catalans*, however, he acknowledges technical backwardness, lack of capital owing to the family nature of Catalan industrial organization, "Catalan cork imperialism" which produced profits for Andalusian and Extremaduran landlords, but not for workers, and resistance to the introduction of machinery, which resulted in the loss of Catalonia's foreign markets by the end of the century.⁷ The two descriptions are not necessarily inconsistent with each other, but they are certainly different in tone and implication.

A certain overpolemical tendency can be illustrated by his objection to a sentence of the French historian Pierre Vilar concerning the restored absolute monarchy in 1814. Vilar wrote that "la masse de l'Espagne noire triomphait de la minorité éclairée."⁸ Vicens, incorrectly assuming that the singular form "la masse" must mean "the masses," objects that the masses were not involved in plotting the restoration of absolutism, that this was merely the first of many *pronunciamientos*, and that Ferdinand was simply taking the path of facility, rejecting the new and returning to the pre-1808 system. But in the following paragraph he writes that the 1814 government was of poor quality, that not even royalist reformers, let alone liberals, supported it, that conservative historians have called it a parody of true national government, and that it engaged in terroristic persecutions of the *afancesados* and constitutionalists. Surely Vicens' own judgment is consistent with Vilar's statement that "la masse," the bulk, of "black Spain" triumphed over the enlightened minority.

None of these critical comments are intended to deny the great value of Vicens' writings on the nineteenth century. He constantly warns the reader of the gaps and the questionable reliability of his statistics, and he is the first Spanish historian of the twentieth century to offer his readers the full panoply of data

⁶ *Id.*, *Historia social y económica*, V, 258-60.

⁷ *Id.*, *Els Catalans en el segle XIX*, 70-71.

⁸ *Id.*, *Historia social y económica*, V, 343. Vilar is quoted in the original French.

which make possible such internal questions as I have just raised about his generalizations. Whatever the errors of detail, he performed an immense service in demonstrating that the nineteenth century was not simply a series of incoherent *pronunciamientos* and civilian cabals and that an authentic peninsular economy and culture made greater strides than at any time since the fifteenth century.

The data and subject matter of *Els Catalans* are much the same as those of his nineteenth-century chapters on Spain generally. It is thus instructive to note the ways in which he distinguishes Catalonia within Spain. He finds a much greater concentration of the bourgeois spirit in Catalonia, whose inhabitants were conscious of the more "European rhythm" of their life and undertook to introduce bourgeois ideals, methods, and organization to the rest of Spain. Emphasizing the strength of the small-family-business tradition, he notes that agricultural and industrial progress in Catalonia was the collective result of numberless small initiatives, that local government was less oppressive because of the virtual absence of a landed aristocracy, and that the leading bourgeois of 1900 were the sons and grandsons of peasants and artisans.

In both his fifteenth- and nineteenth-century writings Vicens is deeply concerned about the proper role of Catalonia. In both eras he sees it as economically the most advanced portion of the peninsula, and also, for that very reason, more socially democratic, less hierarchical and military minded, than the rest of Spain. In both eras also it is more European and Mediterranean than Castile, and just as Catholic. But Catalonia, in his view, must resist the temptation of separatism. In discussing the reign of Juan II, the *remensa* struggle, and the Barcelona revolution of 1462-1472, he clearly sides with those Catalans who saw that their own *patria chica* would be better served by the victory of Juan II than by an oligarchical republic which would be at the mercy of France and Castile. Social justice was also served by the Trastámara victory, inasmuch as the Barcelona oligarchs were oppressive landlords and the *remensas* were in fact relieved of the worst feudal abuses by Ferdinand the Catholic after the general pacification of Catalonia. As for the nineteenth century, and by clear implication the present and future, Vicens writes proudly of the economic initiative, the bourgeois and democratic spirit, and the literary culture of Catalonia. But Catalan prosperity and civil peace in the peninsula depend upon Catalonia's willing loyalty to the sovereign Spanish state.

It remains to discuss briefly the third facet of Vicens' writings: his interpretations of the long course of Spanish history, embodied in *An Economic History of Spain* and in the brilliant, extremely compact *Approaches to the History of Spain*.⁹ Throughout *An Economic History of Spain* Vicens applies a sympathetic

⁹ *Id.*, *Approaches to the History of Spain* (Berkeley, Calif., 1967), is a translation of the second edition of *Aproximación a la historia de España* (Barcelona, 1952, 1960). The translation, by Joan Connelly Ullman, is excellent, and her bibliographical and technical footnotes will be invaluable to all students of Spanish history.

and critical imagination to the interpretation of statistical data. He is always interested in the relationship of economics to the way in which living people act. Contrasting peasants with nomadic herdsmen (two of the most important social classes in medieval Christian Spain) he notes that peasants think in terms of limited resources, tireless labor, and a family-based, largely nonmonetary economy; he sees in herdsmen tendencies toward collectivism, discontinuous effort, and less immediate concern with the value of land itself (pp. 28–29). In discussing the seignorial economy of northern Spain during the Middle Ages he relates price data to a suggestive hierarchy of economic values: at the top, luxury artisan goods such as silver chalices, ivory coffers, and brocades; then, horses, as the key to military and social status; next, furs and livestock; then, land, relatively low in value because of the sparse population and the availability of empty lands during the Reconquest; finally, foodstuffs, which had little or no money value and no prestige connotations (p. 135).

Where earlier historians, with the notable exception of Claudio Sánchez-Albornoz, had treated Castilian history almost entirely in military, ideological, dynastic, and constitutional terms, Vicens relates the economic and geographical factors to the previously well-known political data. From the thirteenth to the fifteenth century, when Castilian hegemony was gradually established in the peninsula, prosperity and demographic growth were based upon a continuing wheat economy and a rapidly expanding wool export trade. The peripheral ports in Santander, the Basque country, and Andalusia were the greatest financial beneficiaries through their control of the export trade. At the same time Castile (in contrast to Catalonia) developed no native urban bourgeoisie, and its infrastructure of cities and communications remained backward relative to those of northern Italy or France. The landed aristocracy remained dominant through Andalusian land grants and a near monopoly of high offices in the *Mesta*, the Church, the municipal and royal councils.

The sort of illuminating and challenging generalizations which are used to interpret the data in *An Economic History of Spain* form the very substance of *Approaches*. The book should be read as an extended “state of the question” essay concerning all the vital issues of Spanish history. To illustrate from the earlier and less well-known periods, Vicens speaks of Christianity as an element of urban dissidence during its first three centuries, and thereafter as the bulwark of Roman authority and unity. He notes that with the establishment of the Visigothic capital at Toledo, the *meseta* became for the first time the political center of Spain. He points to the importance of Mozarabs in León and of Franks in Catalonia. Much-needed monographs are waiting to be written in order to test and refine these interpretive judgments. And Vicens meant this book to be not a manual of either facts or accepted dogmas, but a set of suggestions for his col-

leagues and successors. In summary, as an individual historian D. Jaime Vicens Vives left a magnificent triple legacy to all historians of Spain and the Hispanic world: the detailed political and social history of the Trastámara kings of Aragon-Catalonia; the political and economic history of nineteenth-century Spain; and a series of interpretations which will challenge and fructify all research in Spanish history for many decades to come.

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General

HISTORY: THE LAST THINGS BEFORE THE LAST. By *Siegfried Kracauer*. (New York: Oxford University Press. 1969. Pp. xii, 269. \$7.50.)

DER MENSCH IM BILD DER GESCHICHTE. By *Hanno Helbling*. [Erfahrung und Denken: Schriften zur Förderung der Beziehungen zwischen Philosophie und Einzelwissenschaften, Number 30.] (Berlin: Duncker & Humblot. 1969. Pp. 86. DM 12.80.)

WIDER DIE ÄCHTUNG DER GESCHICHTE: FESTSCHRIFT ZUM 60. GEBURTSTAG VON HANS-JOACHIM SCHOEPS. Edited by *Kurt Töpner*. ([Munich:] Bechtle Verlag. 1969. Pp. 322.)

SIEGFRIED Kracauer's posthumous book is a real gem that will take its place with Marc Bloch's *The Historian's Craft* and E. H. Carr's *What Is History?* as one of the significant essays of recent decades on the nature of history. Kracauer develops a unique position between the attempts of the "neo-positivists," particularly Bloch and the French *Annales* historians, to reconstitute history as a rigorous social science, and the neorealist insistence of Dilthey and Rickert on the nonscientific, idiographic character of history. History, Kracauer insists, is an autonomous discipline basically different in intent and method from philosophy and science, but also from the arts. The nature of the human world as a sphere of freedom and spontaneity makes it impossible to reduce history to the "entreprise raisonné d'analyse" that Bloch desires and makes narration the only possible form of historical discourse. But if Kracauer points at the limits of scientific history, he at the same time rejects the subjectivism and presentism of Croce and Collingwood, which fails to understand that the historian is not only a member of his time but also of the time he investigates. Historical knowledge requires "active passivity." The historian must let the past speak to him rather than, as Bloch and Collingwood suggest, approach it through hypotheses. On the other hand, Kracauer finds Namier's micro-history wanting because "historical reality resides not only in detail, biographical or otherwise, but also extends into the macrodimension," into "long-enduring events," such as wars, social or religious movements, or slow institutional changes. History indeed is not totally unstructured but the "patterns, strands, and sequences thread a material which is for long stretches inchoate, heterogeneous, obscure." The historian thus requires ideas that "connect the particular with the general in an articulate and truly unique way" but that, unlike philosophical ideas, make no claim of finality, completeness, or universal validity. Both macro- and microhistory are therefore necessary, but the two can never meet. The accumulation of details never leads to ideas. General history on the other hand remains subjective; the more general it is, the less concrete is its historical vision. Progress in historical knowledge is thus impossible. At its best, history, like the film, "concentrate[s] on close-ups and from them casually range[s] over the whole, assessing it in the form of *aperçus*." The model of such history Kracauer sees in Burckhardt.

I wonder, however, whether the fact that the historian "invariably comes across irreducible entities" of freedom needs to lead to Kracauer's position that rational analysis

has no place in historical study. Kracauer, I believe, never fully understood the position of Bloch or the *Annales* historians. Fully aware of the nonrational components in human action, Bloch and his disciples nevertheless realized, as Kracauer did, too, that historical behavior is never without some structure and coherence. "Incalculability," Max Weber suggested, "is the privilege of the insane." On these elements of structure Bloch sought to build a rational science utilizing generalizations of limited scope, not that different in intent from Kracauer's "historical ideas" but subject to validation and revision. Kracauer, on the other hand, by insisting that "historical ideas," which he considered the products of "intuitive leaps," were not only subjective and personal but also incapable of validation, arrived at a position not far removed from the subjectivism and aestheticism he condemned.

Helbling's brief volume in a rather abstruse language goes over the well-known existentialist arguments for the historicity of man and the perspectivity of all historical knowledge. Helbling recognizes no criteria by which the relative objectivity of such knowledge can be judged. Each perspective is a matter of fate, and all perspectives are ultimately of the same value.

Wider die Ächtung der Geschichte is a collection of essays in honor of a somewhat unique member of the German historical profession, Hans-Joachim Schoeps, a Jewish theologian involved in the youth movement in the last years before 1933. Soon after his return from exile after the war, he assumed a chair at the University of Erlangen where he became an outspoken defender of Prussian traditions. The essays relate to three aspects of Schoeps's interests: his Judaism; his attempt to create a new discipline, "Geistesgeschichte," which would seek to re-create the *Zeitgefühl* and *Lebensbewusstsein* of an epoch; and, finally, his political conservatism. For the historian the discussion and application of the methods of "Geistesgeschichte," especially in Ernst Benz's essay on oral history and Bruno Seidel's excellent essay on the social milieu of the underprivileged in the eighteenth century, are of some interest. The conservative political intention of the volume is made clear in the essays by Kurt Töpner, Karl Kupisch, and Hans-Viktor Schulz-Klingauf. All three regret the loss of historical consciousness in Germany, the break in continuity, the re-education imposed by the Allies after 1945, and the spiritual shallowness of the *Bundesrepublik*. Schulz-Klingauf in the book's concluding essay goes beyond this to an attack against parliamentary democracy and a call for an elitist society for tomorrow with planned breeding reminiscent of Plato's *Republic*. The volume is of more interest as a historical document of one, fortunately small, aspect of the *Geistesgeschichte* of the Federal Republic than it is of value to the historian.

State University of New York, Buffalo

GEORG G. IGGERS

VOX POPULI: ESSAYS IN THE HISTORY OF AN IDEA. By *George Boas*. [Seminars in the History of Ideas.] (Baltimore: Johns Hopkins Press. 1969. Pp. xv, 292. \$2.75.)

THIS is a learned and graceful collection of essays. The Latin tag, *Vox populi vox dei*, is certainly familiar enough, but I, for one, had no notion that the phrase first appeared—quoted with disapproval—in a letter from Alcuin to Charlemagne written in 798. Many other delicious and sometimes surprising quotations follow, ringing the changes on the notion that the people, variously conceived, have a voice, somehow recognizable, that is endowed with authority superior to ordinary human expressions of knowledge or of opinion.

Professor Boas writes in the humanist tradition. The classics and the Bible are the

sources he turns to for origins; medieval Latin, French, English, and American literatures are the mines he exploits for examples of the changes that have occurred in the cluster of ideas in which he is interested. In addition, he devotes a pair of chapters to "The People in Art" and "The People as Artist" for which paintings provide his data. Eastern Europe and the non-Western world do not exist for Boas; Germany he views as a twilight zone where *Zeitgeist* and *Volkseelen* haunt the landscape in quite unnecessary profusion.

The main value of this book arises from the wry challenges it offers to commonplaces of American political rhetoric. Boas makes it abundantly clear that he does not share the view that ignorance produces wisdom as long as it is shared by a sufficient number of people. As he says in closing: "One turns back to the first book of Samuel [for an account of Saul's election to kingship] and one realises that collective opinion will always overcome reason and common sense, in the future as it has now for some 2500 years."

As history, however, these essays fall short. The author disarms criticism by disclaiming sufficient knowledge for the task of writing a real history of his theme and settles instead for essays "indicating high points in the narrative." He does not try to account for changes he finds because he doubts that explanation is possible "when one is dealing with individual occurrences and not with classes of events."

Since the books and pictures from which he draws his material are individual occurrences, explanation becomes, by definition, impossible, or, at least, something a prudent philosopher like Boas reserves for naïve and temerarious historians, like me. But an anthology of quotes and paraphrases, arranged thematically rather than chronologically, is not history, even when glossed with ripe and nimble erudition, as is here the case. A better subtitle would be: "Essays on the Permutations of an Idea."

University of Chicago

WILLIAM H. McNEILL

HISTORY AND SOCIAL THEORY. By *Gordon Leff*. (University: University of Alabama Press. 1969. Pp. vi, 240. \$6.50.)

THE study of history, according to Leff, has too often been left to metahistorical speculation or analytical philosophy. Each reduces history to something which it is not. Instead, the author proposes to inquire into the shape of the discipline and what it owes to philosophy and the social sciences. To this admirable task, he brings with him the historian's commitment to the actual procedure of historical research and reconstruction, as well as to the Dilthey-Rickert-Weber tradition of distinguishing the human studies from the natural sciences.

There are two parts to the book. Part One is a study of the problem of historical knowledge, emphasizing the distinction between history and the past. History, as present reconstruction of a past, deals with the specific and the contingent, rather than the universal and the causative. Hence, it is the understanding of the individual within a context—something quite different from the current philosophical preoccupation with historical explanation and a covering law model.

But the reality of a past which the historian studies is a "mind affected" human world. Central to the understanding of that world is "the tension, or discrepancy, between reality and man's conception of reality at any given time." And, in the second part, Leff studies that conception as a problem in ideology. He criticizes both the Marxist notion of class ideology, and the Mannheimian concept of total ideology. Instead, he approaches this ideology in terms of the Weberian system of interaction among economic

class, group status, and political power. "Hence there is never only one level of understanding in an ideology; it is the product of different standpoints and experiences."

The book is thoughtful and stimulating. Few practicing historians have taken the trouble to be informed in both philosophy and social theory—an admittedly Herculean task. Keeping in mind the immensity of this present task in historiography, Leff has succeeded well. His book is far superior to the sort of dated, neopositivistic work of E. H. Carr. And certainly, from the historian's standpoint, its many insights are much more relevant than any currently contained in analytical philosophies of history.

But, beyond this, I must raise certain reservations. In Part One Leff criticizes the analytical approach to historical knowledge; whereas, in Part Two he criticizes the Marxist-Mannheimian concepts of ideology. I believe that the first part is much more successful than the second. The critique of analytical philosophy of history has current relevance, since it is a real opponent in Anglo-American academic circles. And Leff has succeeded in showing that the old distinction between history and nature is actually more relevant to historiography than the analytics' preoccupation with the narrative sentence. On the other hand, his study of man's conception of reality as a problem in ideology seems a bit dated. Methodologically, Weber himself was unable to reconcile the use of objective categories to understand subjective motive. And present-day social theory in its study of intersubjectivity has advanced beyond Weber.

Historical knowledge presupposes the antinomies between past and present, between objective reality and subjective awareness of that reality. Leff advances this insight in his book, and I wholeheartedly support it. The acceptance of both of these antinomies leads to what Leff would call "the time-lag of man's world." But time lag is neither a problem nor a paradox; it is the framework of man's intersubjective world, and it can be elucidated in terms of the contemporary philosophy of time consciousness.

San Francisco State College

DONALD M. LOWE

PORTUGAL AND BRAZIL IN TRANSITION. Edited by *Raymond S. Sayers*. (Minneapolis: University of Minnesota Press. 1968. Pp. 367. \$11.00.)

IN 1966 scholars from a variety of disciplines with a common interest in the civilization of the Portuguese-speaking world, largely from the United States but also from Portugal, Brazil, the United Kingdom, France, Spain, Canada, Czechoslovakia, and Chile, came together for a week of study and discussion, first in Cambridge and then in New York, at the Sixth International Colloquium on Luso-Brazilian Studies. They came primarily at the invitation of Professor Francis M. Rogers, to continue the tradition that Lewis Hanke had happily begun in 1950, and to ask the question that in many instances was left unanswered, "Where are we in Portuguese and Brazilian studies and where do we go from here?"

Rogers went out of his way to invite young and untried scholars in the field, optimistically believing that they might be able to tell us along what intellectual lines we were destined to develop, but what we got from them was essentially indignation and the song of the Lusitanian bogey. Actually, the direction of Luso-Brazilian studies during the next twenty years, which the colloquium was meant to identify, was not always directly considered, even though every paper, consciously or unconsciously, at least suggested areas of scholarship that needed further investigation.

It is good to have this record of an interesting and at times exciting international meeting. Four years removed from their preparation, the papers still retain a freshness and a vitality that speak well of the competence of the organizing committee and the

editing of Mr. Sayers. I enjoyed the papers on literature, especially the one by Jorge de Sena, John H. Perry's delightful piece on "New England and the Portuguese World," Alexandre Lobato's reflective essay on "Permanence and Change in Overseas Portuguese Thought," Florestan Fernandes' "Economic Growth and Political Instability in Brazil," José Augusto França's "Lisbon of 1750-1850," with its emphasis on the city that arose out of the havoc created by the earthquake of 1755, and the studies on the history of music, above all Robert M. Stevenson's fascinating "Portuguese Music and Musicians Abroad to 1650." I also appreciated the lively touch provided by the remarks of the commentators and by selected comments from the floor.

Dispersive though the book naturally is—there is something in it for every taste—the editing was done so felicitously that it will be hard to come upon a more engrossing volume of proceedings. Moreover, the book is a joy to look at, a printing achievement that must be emphasized. I welcome it as a worthwhile addition to the literature of the larger subject and recommend it highly to the specialist and the intelligent layman alike.

Catholic University of America

MANOEL CARDOZO

RUSSIA, STATI UNITI D'AMERICA E REGNO DI NAPOLI NELL'ETÀ DEL RISORGIMENTO. By *Vincenzo Giura*. [Collana di storia del commercio, Fonti e Ricerche, Number 2. Istituto Universitario Navale di Napoli, Facoltà di Economia marittima, Istituto di Storia del Commercio, Number 2.] (Naples: Edizioni Scientifiche Italiane. 1967. Pp. xv, 362. L. 4,000.)

THE observation, quoted by the author, that the kingdom of Naples "only subsisted by grace of a precarious equilibrium" might be taken as adequate summation of his findings. From the eve of the French Revolution to its final disappearance within a united Italy that state was indeed an obsolete survival.

Russia took an interest in the preservation of the Italian political structure and in some measure assumed a protective stance, especially toward the Neapolitan kingdom, even though ultimately England mattered more. This lends some interest to the economic connection between Naples and Russia, which is assiduously traced in minute detail here. Agricultural states that they both were, there was yet some basis for exchanges—grain for citrus fruits and olive oil in large part—between them. The volume of these exchanges was never very large and in addition was impeded by a series of accidents and crises, sometimes political and sometimes natural. We learn as much about Russian trade as about that of the Two Sicilies.

The internal conditions of the Neapolitan state presented an equally serious handicap. Its fiscal and commercial policy, the lack of a competent mercantile class, and an inefficient bureaucracy all revealed its lack of progress; the major part of its Russian trade was carried in foreign bottoms. These same limitations, on the Neapolitan side, applied to trade with the United States, but the smoother and steadier course of the latter compared with that of Russia resulted in a steadier growth of exchanges, to be sure also of small dimensions.

In a succession of chapters, following in chronological order, first the one (Russian) then the other (American) situation is traced. The treatment is scholarly, and statistics abound, almost to the point of being redundant. A question may arise concerning the validity of so much effort over so relatively small a matter, and the author himself acknowledges that a larger framework, the whole trade of the Two Sicilies, would have been desirable. The lack of adequate information is his plea; certainly he has made

exhaustive use of that available, even though it, too, has limitations. But knowledge is made up of small accretions, and even a negative experiment has value.

Columbia University

RENÉ ALBRECHT-CARRIÉ

LA GUERRA DEL 98. By *Pablo de Azcárate*. [El Libro de Bolsillo. Sección: Humanidades.] (Madrid: Alianza Editorial. 1968. Pp. 219.)

THE stated purpose of this volume is to give Spanish people a proper perspective on the war with the United States, which, the author insists, had been relegated in the thinking of his people to a mere "intermediate situation between history and the present," and therefore its significance is less understood than many remote events of less importance. Responsibility for this he places on the group of intellectuals known as the "Generation of 98" who took the view that the war was a national disaster brought on by "old defeats, corruption, immorality, incompetence, and favoritism in high places," and were putting on a campaign to renovate their nation. In so doing, the author thinks, they overlooked many factors in the causes, course, and results of the war. He does not minimize the faults of the Spanish government or misrule in Cuba and the other colonies, which he says justified the rebels, whom he goes so far as to praise, but he insists that there was always a chance for satisfactory agreements with them that would have provided liberty within the Spanish Empire had not the United States given encouragement to the rebellious minority in order to further their desire to gain control over Cuba.

Material for the survey of the causes, course, and negotiations at the end of the war and for support of the author's thesis is drawn from works by Spanish writers and from publications in the United States such as H. E. Flack's *Spanish-American Diplomatic Relations Preceding the War of 1898*, F. E. Chadwick's *The Relations of the United States and Spain: The Spanish-American War*, and *Papers Relating to the Foreign Relations of the United States, 1898*. From the American works Señor Azcárate obtained support for his criticism of United States policy, and he makes out a good case for the diplomatic and legal correctness of Spain's diplomatic procedure before and after the war. Since this volume, first published in 1960 and now circulating in a pocket edition, will do much to shape future Spanish interpretation of the war, it merits careful attention by historians in this country.

Asbury College

DUVON C. CORBITT

GERMANY AND THE OTTOMAN EMPIRE, 1914-1918. By *Ulrich Trumpener*. (Princeton, N. J.: Princeton University Press. 1968. Pp. xv, 433. \$12.50.)

DEFEATED states, like defeated candidates, are often quickly forgotten. As might-have-beens, they cannot frame either postelection or postwar policies. In the Middle East at the close of World War I it was Britain, France, and the Turkish Republic—not Germany, Austria, and the Ottoman Empire—that arranged the peace settlement, and that settlement shaped the later course of the area's history. Still, the German-Ottoman alliance of 1914-1918 significantly influenced the war itself. The closing of the Straits weakened the tsarist military effort and *Entente* coordination, and the thrusts toward Suez tied British troops down in the eastern Mediterranean. For far too long the German-Ottoman alliance has received only marginal attention, largely seen through enemy, and therefore necessarily slanted, documents. An authoritative analysis is overdue. Ulrich Trumpener has gone some distance toward satisfying this need.

The value of Trumpener's work lies in meticulous scholarship that rests on a syste-

matic and comprehensive search of the German Foreign Office files. But more private papers might have been used, and the Foreign Ministry archives of Austria-Hungary, the reluctant adherent of the German-Ottoman alliance, might have been more than sampled. The standard published works in Western languages have all been consulted, and a few books in Turkish thrown in. The author tells us that he was denied access to the Turkish State Archives. We are the losers, for if he had been able to process the Ottoman testimony with the care given the German materials, he would have produced a more authoritative book. To put it differently, Trumpener gives us an action-packed movie of German alliance policy making but only a still-life, impressionistic view of the Sublime Porte, ultimately reconstructed from the chance availability in the German records of informed reports on Ottoman behavior.

The author leaves the evaluation of the fighting to the military historians. Otherwise, he explores the central issues of the wartime alliance: the slow-motion conversion (August–October 1914) of the formal alliance into a real one, an assessment of the top German officers assigned to Ottoman duty, the tough bargaining of Ottoman politicians and diplomats, the German quest for postwar economic preference in the Ottoman Empire, German failure to moderate the brutal Ottoman policies on the Armenians, and German muddling on the Baghdad Railroad. All these topics, including the collapse of the alliance in 1918, are examined on the whole with mature judgment and convincing argument. The value of the book is enhanced by an excellent bibliography.

Columbia University

J. C. HUREWITZ

L'ARMISTICE DE RETHONDES, 11 NOVEMBRE 1918. By *Pierre Renouvin*. [Trente journées qui ont fait la France.] ([Paris:] Gallimard. 1968. Pp. 486. 30 fr.)

STOPPING a war is as complex a political-military process as starting one. Pierre Renouvin uses the 1918 armistice to focus upon the thorny and topical questions raised by the way World War I ended. Was the German Army stabbed in the back? If so, Ludendorff wielded the dagger. Should the Allies have gone on to Berlin? Few officials (for example, Poincaré and the American generals) opposed an armistice, and they were mollified by the eventual terms. How did the "Bolshevik menace" shape the armistice? Renouvin concludes that the Allies drifted, and that anti-Bolshevism served equally the proponents of a lenient armistice and a tough one.

This is a work of *haute vulgarisation*. Readable but eminently scholarly, shunning anecdotal trivia, it is a reproach to the American "The Day That . . ." genre.

There is an international selection of documents and an up-to-date evaluation of sources. Renouvin scrutinizes the German side as closely as the Allied, analyzes masterfully the military situation on both sides, and gives the eastern theater, at last, due balance with the western. He used French military archives, the last of the major belligerents of 1918 having opened its papers. These yielded new material on the Allied offensive planned for mid-November, and filled Arno Mayer's lacunae on the prearmistice positions of French public figures. *Le doyen* Renouvin has not rested on his laurels.

There is little national flavor, considering that Renouvin was already twenty-five at the time. Foch equals Ludendorff in villainy, but Renouvin takes a moderate view of French war aims and intervention in Russia. He omits the French share in undermining the Prinkipo negotiation.

Renouvin uses the work of revisionists without adopting their priorities. As compared, say, with the early pages of Mayer's *Politics and Diplomacy of Peacemaking*, the book focuses clearly upon decision makers at the top, and, while defense of the so-

cial order figures largely in their motives, the author rests their decisions ultimately on a close analysis of evolving military posture and strategic goals. It is an authoritative, traditional synthesis.

Columbia University

ROBERT O. PAXTON

THE FLAME OF FREEDOM: THE GERMAN STRUGGLE AGAINST HITLER.

By Eberhard Zeller. (Coral Gables, Fla.: University of Miami Press. 1969. Pp. 471. \$15.00.)

HITLER'S PLOT TO KILL THE BIG THREE. By Laslo Havas. Translated from the Hungarian by Kathleen Szasz. Revised edition, with additional material translated by Jean Ure. (New York: Cowles Book Company. [1969.] Pp. 280. \$5.95.)

WHATEVER the ultimate effect of the assassination of Churchill, Roosevelt, Stalin, or Hitler in 1943 or 1944 might have been, the immediate impact of the death of any of them would probably have been very great. These two books deal with attempts on their lives.

The Flame of Freedom is a competent translation of the fourth edition (1963) of a standard work on the resistance to Hitler. Although it offers nothing essentially new and its treatment of the early part of the war is superseded by Harold C. Deutsch's *The Conspiracy against Hitler in the Twilight War* (1968), it fully deserves to be received here, as it has been in West Germany, as a reasonably detailed and admirably balanced synthesis for the serious student.

In radical contrast to Zeller's clearly conceived, judiciously written, and (even in this translation) carefully annotated work, the "journalist, screenwriter, and acknowledged expert on subversive warfare" Laslo Havas offers in *Hitler's Plot to Kill the Big Three* a tale of lurid adventure, embellished with romance, rape, and violent death, which, to continue quoting the dust jacket, "reads with all the dramatic suspense of an Ian Fleming spy story." Unfortunately, he gives little evidence that it is more than just that. There is no documentation, and the bibliography, with entries such as "*Documents on German Foreign Policy, 1918-1945*, Washington, 1949," is unconvincing.

Havas contends that a serious attempt to assassinate the Big Three at Tehran late in 1943 was foiled because the German agents happened to get involved with an Anglo-Swiss double agent who played along with them until, after many exciting pages, they were finally taken care of. The fact that what Havas writes is neither plausible nor documented does not make it untrue. The problem is that his is only one of several mutually contradictory versions of the Tehran plot. In 1965 the Russian Alexander Lukin published an article (cited by Havas) in which a large-scale German operation was reported to have been suppressed by the Russians, though, out of alleged concern for Roosevelt's security, they invited him to stay in the Russian embassy compound in a thoroughly wired building where they could record every word spoken. The Hungarian-born author Ladislav Farago told me early this past July that he is planning to publish another version of the plot in his forthcoming book on the history of German intelligence in the Second World War. Anticipating an assassination attempt at Tehran, Farago maintains, the British, through double agents, initiated and organized an elaborate plot under their own surveillance. When the Axis agents, assembled in their trap, were finally seized on the eve of the conference, it was too late for further action to be initiated by the enemy, and so the Big Three were safe.

Presumably Havas was unaware of Farago's yet unpublished interpretation, but he does discuss the Lukin article, which he dismisses as absurd, citing the similar judgment

of Otto Skorzeny, who plays an important role in his book, having by no means been engaged only in Mussolini's abduction (or liberation, depending on one's point of view) from the Gran Sasso. When I asked Skorzeny about the Lukin, Havas, and Farago versions in Madrid late in July, he explained that, although serious consideration had been given to the possibility of an action at Tehran in 1943, he had to recommend that it be abandoned. Not only was it impossible to get the kind of information on which such an undertaking would have had to be based, but the logistical problems were virtually insuperable. He cannot prove there was no plot, but he cannot imagine that a serious action would have been undertaken without his having learned of it sooner or later.

If Skorzeny agrees with Havas that Lukin's version is false, he also agrees with Farago that Havas' is little better. Havas has interlaced his tale with accurate details, but included so many gross errors (as Skorzeny convincingly explained) that his book cannot be taken seriously. Until a carefully documented account of the Tehran plot is published, perhaps Farago's, we can only reserve judgment.

Southern Illinois University

DONALD S. DETWILER

Ancient

ROMAN DOCUMENTS FROM THE GREEK EAST: *SENATUS CONSULTA* AND *EPISTULAE* TO THE AGE OF AUGUSTUS. By Robert K. Sherκ. (Baltimore: Johns Hopkins Press. 1969. Pp. xii, 396. \$17.50.)

THIS book fills a long-standing need, especially for the student of Rome's relationships with and policy toward the Greek East. It is an up-to-date corpus of the *senatus consulta* and letters of Roman officials written in Greek and preserved on stone, from the period of the Republic and the Age of Augustus (although one, Number 32, of A.D. 23 is inexplicably included). Much new material has come to light since 1888, when the only other work of this type was published by Peter Viereck; Professor Sherκ fell heir to his notes for a projected second edition. Fifty-three of the seventy-eight documents included here were not, however, in Viereck's *Sermo Graecus*.

The material is presented in two sections: "*Senatus Consulta*" and "*Epistulae*." Each category is introduced by a concise and informative discussion of the form, style, and language used; every document is preceded by a full bibliography and a description of the stone. After the Greek text come an *apparatus criticus* and a commentary; there is no translation. The author is generally conservative in his comments, being content to give the historical background, indicate the problems, and present conflicting viewpoints, although at times he offers his own contribution. There are full indexes.

Historically, then, the book is well written and useful, but, epigraphically, there are serious deficiencies. There is much inconsistency in the descriptions of the stones. Often the spot where they were found is omitted; more often, the present location. In two cases there is no description at all. Letter forms are generally ignored, even in one case where the date is argued from them (Number 71). Photographs would have been very useful to the reader (there is but one, of a squeeze), but probably they would have greatly increased the price. The author states that: "In the establishment of the text the matter of control was considered vital," and it is rather a shock to find that in only twelve cases have squeezes been consulted and in only one (Number 43) was the stone itself examined; even there, the *apparatus* shows little evidence of autopsy. Although published photographs were used when possible, they certainly are no substitute for the

stones themselves; it is unfortunate that more could not have been examined.
University of Cincinnati DONALD W. BRADEEN

INDIVIDUALS IN THUCYDIDES. By *H. D. Westlake*. (New York: Cambridge University Press. 1968. Pp. x, 324. \$10.50.)

THE thesis of this interesting and lucidly written book is that Thucydides' view of history developed toward greater emphasis on the role of the individual during the years the historian devoted to the composition of the *History*. The author considers separately the two parts of the work, Books 1-5.24 and 5.25-8, respectively, or, roughly, the Archidamian War, on the one hand, and the Peace of Nicias, the Sicilian Expedition, and the Ionian Revolt, on the other. He has chosen for study twelve individuals, some major, others minor; six are Athenians (Pericles, Phormio, Cleon, Nicias, Demosthenes, Alcibiades), and six are Spartans (Archidamus, Cnemus, Alcidas, Brasidas, Gylippus, Astyochus). This symmetry cannot, however, conceal the lesser importance of most Spartan sketches when held against Thucydides' presentation of Athenians. Each individual is discussed in such detail that the book is an important study of the Peloponnesian War as well. Westlake supports his case by frequent stress on Thucydides' method of guiding the reader's judgment by means of the dramatic presentation of evidence. He also points out that in the second part Thucydides gives much larger scope to private conferences and private motivations than to public meetings, which are so greatly elaborated in the first part. Even the public speeches of the second part reveal the personality of the speakers more clearly than do those in the first. The author is well versed in contemporary scholarship, and the book is very useful as a survey of scholarly opinion on such questions as the role of Demosthenes at Pylos, of Cleon at Amphipolis, or of Nicias in the Sicilian Expedition.

Despite the excellence of the book, some criticisms are perhaps in order. First, the main thesis needs perhaps more qualification than the author allows. By omitting the character sketches of Themistocles and Pausanias on the grounds that they deal with persons already dead, the author has weighted the evidence in his favor (see p. 4, n. 1). As Westlake himself admits, the treatment of Cleon and Brasidas in the first part is, for different reasons in each case, exceptional. In the second part, Thucydides does not handle the figure of Demosthenes during the Sicilian Expedition altogether in the new manner. It is then a question of relative emphasis and of a gradual transformation of Thucydides' style. Secondly, Westlake explains the shift of emphasis biographically with reference to the composition of the different parts of the *History*: when in exile, Thucydides had less opportunity to obtain information on public debates and more opportunity for contact with private individuals. (Alcibiades himself, or persons close to him, were at one time a main source of information; see, for example, p. 232.) Thus he came to see that individuals, rather than public bodies, had a profound influence on events. But a "unitarian" interpretation of this shift, which has been clearly demonstrated by Westlake, is at least possible. The influence of individuals on events, as described in the second part of the *History*, is primarily harmful, as is especially evident in the cases of Nicias and Alcibiades. The rise of individualism in the latter part of the Peloponnesian War, which is accompanied by decreased control by public bodies such as the Athenian assembly and, in general, by greater reliance on emotion rather than reason, is a phenomenon of decay, and I believe that Thucydides presents it in this way. From this point of view the notorious treatment of Cleon in the first half of the work does not result entirely from a personal grudge. The fragmentary nature of the

History makes such judgments difficult to prove, of course, and I do not mean to suggest that there are no changes in the different sections of the work. But an author's later thought may incorporate, rather than supplant, his earlier views: a direct line of development can be drawn from Pericles via Cleon to Nicias and Alcibiades. I believe Professor Westlake would agree with this last statement.

Finally, the emphasis on individualism in Thucydides is presented as an improvement in historical technique (or so I read the book). This is, however, a question of intellectual history rather than of history as such: Thucydides stands in the middle between the fifth- and fourth-century views of the relation between individual and society. One wishes the author had given greater attention to the development of ideas between, say, Herodotus and Plato; the brief and rather unsatisfactory discussion of a few ancient historians that begins on page sixteen is no substitute for such an analysis. I also miss a discussion of Thucydides' view of the nature of individual character (Thucydidean psychology) in a separate chapter. From the point of view of the history of ideas, Westlake treats Thucydides too much as if he were a modern historian.

The book is, nevertheless, highly recommended as a clear and stimulating presentation of the major figures in Thucydides and of his methods for dealing with them.

University of North Carolina, Chapel Hill

HENRY R. IMMERWAHR

CICERO: EIN MENSCH SEINER ZEIT. ACHT VORTRÄGE ZU EINEM GEISTESGESCHICHTLICHEN PHÄNOMEN. Edited by *Gerhard Radke*. (Berlin: Walter de Gruyter & Co. 1968. Pp. 259. DM 28.)

THIS is a collection of eight lectures on Cicero delivered by various German scholars at a conference of educators held in Berlin. Professor Radke introduces the volume briefly, and Bodo Finger concludes it with a bibliography of Ciceronian scholarship published between the years 1961 and 1964, though random items from 1965 and 1966 also occur. The majority of the essays are primarily surveys of various large and familiar areas of Ciceronian studies. Among these are Professor Budde's review of the known and conjectured portrait busts of Cicero, Professor Kaser's sketch of the workings of Roman civil law, Professor Richter's outline of the main attitudes toward Cicero of Greeks and Romans under the Empire, and Professor Classen's survey of the influence of Cicero's speeches on scholars of the fifteenth and the sixteenth centuries in Italy, Spain, and France. Professor Römisch' attempt to evoke the "environment and atmosphere" of various passages in certain of Cicero's speeches is not happy; the analysis is spotty and superficial, and the conclusions, for the most part, are trite.

Of more interest is Ulrich Knoche's analysis of the first book of *De Legibus*, in which Cicero tried to give a philosophical basis and justification to Roman law. Knoche believes that this was an innovative approach which came as something of a shock to Cicero's contemporaries. Roman law had been regularly treated within the historical framework of tradition and custom (for example, the Twelve Tables and the praetors' edicts). At a time when such appeals were rapidly losing their hold on the national consciousness, Cicero found a new basis for Roman law in the tenets of Greek philosophy. In particular, the notion of *ius commune*, or natural law, served to give Roman law a world-wide perspective and application.

Ciceronian philosophy is further discussed by Professor Otto Seel in a fine essay. Magnificence of language and ease of expression should not, he warns, be equated with superficiality. On the contrary, it is precisely in his *Sprachliebe* that Cicero's philosophical merit lies. Eschewing the narrow, self-contained philosophical "systems" that

so many Greeks espoused (but that seemed to relate to life so slightly), and the antiquarian interests that engaged the attention of men like Varro and Nigidius Figulus, Cicero characteristically turned his attention to men and life as he knew them. His attachment to the Academy and to skepticism is nicely delineated, and in a rousing peroration worthy of the master himself Seel asks whether this is not just the sort of approach needed and desired today: sane, humanistic, and, above all, "relevant."

The most substantial essay in terms of length is by Professor Christian Meier, entitled "Ciceros Consulat." He recapitulates many of the ideas concerning the collapse of the Republic and the nature of Roman political life which he developed in his recent book, *Res Publica Amissa* (1967). The failure to reform the tottering constitution resulted in a "Krise ohne Alternative" for which Meier finds the year of Cicero's consulship, 63 B.C., to be the turning point. But Meier is not much interested in Cicero himself or in his exploits ("Ciceros Consulat ist . . . weit über die Person des Consuls hinaus interessant"), since he was a utopian dreamer who ignored the realities of Roman politics by thinking of political interests and conflicts in the simple moral terms of "good" and "bad." Hence Cicero was an anomaly ("eine Ausnahme von der Allgemeinheit seiner Standesgenossen"); he stood in notable contrast to "die eher pragmatisch denkenden führenden Senatoren." Cicero thus figures little in this lengthy essay on his consulship. With respect to the collapse of the Republic and the nature of political life, however, Meier relies too much on unsubstantiated generalization; in particular, he is overly fond of historical "mind-reading" (the section on Caesar's excogitations is a case in point). With respect to Cicero, moreover, his approach seems ungenerous and abbreviated, and it exhibits something of the simplistic tendency to label things black and white (for which he faults the consul).

The title of the book at first sight seems innocuous enough, but it is inappropriate. Some of the essays are little concerned, if at all, with Cicero's "times." Others ignore or slight "the man" (Kaser scarcely mentions him). Instead of the typical and the expectable, most of the essays tend to stress the innovative and the exceptional aspects of Cicero's life and thought.

Princeton University

T. J. LUCE

PLEBS AND PRINCEPS. By Z. Yavetz. (New York: Oxford University Press. 1969. Pp. ix, 170. \$5.50.)

THE populace of ancient Rome preferred the rule of one man to the oligarchic domination in the Late Republic. To win the favor of the crowd the princeps had to provide food and amusements, and, in addition, he had to act in accord with its elementary ideas of justice and show himself fond of the people. Quite useful in this respect, at least for Caligula and Nero, was their humbling of the upper classes. Yavetz, however, rejects Kornemann's theory that the principate was a *Volksmonarchie*; the plebs was never more than a secondary factor, though its anger could be troublesome.

To support these reasonable views, the author surveys popular action, from Caesar to Nero, both topically and chronologically. He has missed little in the ancient or modern literature, though I failed to find that pregnant line of Ennius that was quoted again by Phaedrus, "To speak aloud will cause a plebeian trouble." The monograph thus may be considered a useful survey of the evidence, though the treatment is not always consistent. On page fourteen, the people as a whole never take part in uprisings, but, on page twenty-five, "the entire *plebs* rose in revolt."

Much further one cannot go. While Yavetz knows modern work by Rudé and others,

he feels that it cannot be applied very extensively to the Rome of the Caesars. This is probably correct, but more incisive analysis within the limits of our information would have been welcome. To give one example, Chapter v is entitled "The Tribunician Power in Shaping the Image of the Principate," but the promise of the title is simply not fulfilled in the assembly of information that follows.

University of Illinois, Champaign

CHESTER G. STARR

Medieval

CONTOURS OF THE MIDDLE AGES. By *Léopold Genicot*. Translated from the French by *Laurence* and *Rona Wood*. (New York: Barnes and Noble. 1967. Pp. xi, 322. \$7.50.)

THIS book is an English translation of the second edition of Léopold Genicot's synthesis of the Middle Ages, which appeared originally under the title *Les lignes de faite du moyen âge*. Since the French version has already received, in various reviews, the critical approval of most medievalists, I shall not comment in detail here on the content of the translation, but shall note Genicot's conceptual framework and his chronological arrangement of the book.

The first part begins with the Germanic occupation of the West and ends in the middle of the ninth century. Much of what Genicot says about this period has a traditional ring, some of it derived from Christopher Dawson. For Genicot, the event of cardinal importance was the crossing of the Rhine and Danube by the Germans, an event, he contends, that marked the advent of the Middle Ages because medieval civilization arose from the "meeting and interaction of Rome, the barbarians and the Church." In what he calls the "dawn" of the Middle Ages, order was gradually imposed over a violent society by the Church and the Carolingians, and there emerged a first rough draft of Western civilization.

The second part concerns the medieval "noon" and embraces the period from the mid-ninth to the early fourteenth century, a period in which the one hundred-odd years after the first quarter of the twelfth century saw the "glorious peak" of medieval civilization. Initiated by the struggle for peace, this period saw the achievement of political stability, it benefited from the spiritual revival of the Church, it profited from the rise of population with the attendant social and economic consequences, and it culminated in the thirteenth century with the synthesis of God and nature. At the center was the Church, which gave medieval culture "its deeper unity and originality," and artists and thinkers their *Weltanschauung*. Unity of culture was more absolute at this point in the Middle Ages than at any other time in history.

The third part, the "dusk" of the Middle Ages, opens with the early fourteenth century when the Church began to retreat before the twin adversaries of the new monarchies and the bourgeois of the towns. Genicot asserts that, because the Church lost its dominance, the previous medieval unity, harmony, and spirit withered away. In the fourteenth and fifteenth centuries nationalism took root, scientific study broke away from Christian theology, sculpture and painting declared their independence from architecture, and literature became secularized, with writers and scholars no longer predominantly drawing their inspiration from Christian faith but from the ideals of antiquity. The Middle Ages ended between 1450 and 1550 when the Reformation, with its heterodoxy, shattered the religious unity of the West, thereby speeding up nationalism, strengthening the trend toward individualism, and favoring the rise of capitalism.

But in this transition the fundamental break between medieval and modern came from the rout of Christian obedience by the modern ideal of liberty.

Genicot's interpretations and conclusions rest upon vigorous inquiry into all aspects of medieval life, inquiry that produced a collection of "curves." After superimposing curve upon curve to see if they were in harmony and discovering that their concordance was extraordinarily exact, he arrived at a single curve. This curve or interpretation may not be new, and there may be those who would argue that Genicot has given the Church too central a position in his curve, that it explains too much; he has, nevertheless, written a superb synthesis of the Middle Ages, and this excellent translation will make it available to many who read only English.

Brown University

BRYCE LYON

DEL CONCEJO MEDIEVAL CASTELLANO-LEONES. By *Maria del Carmen Carlé*. (Buenos Aires: Universidad de Buenos Aires, Facultad de Filosofía y Letras, Instituto de Historia de España. 1968. Pp. 295.)

ONE of the principal consequences of the double-edged process of reconquest and repopulation was the development of the medieval Castilian *concejo* or municipality. In the beginning it was little more than a fortified place whose inhabitants controlled a substantial rural area and cultivated the soil, raised sheep, and engaged in warfare against the Moors. The growth of the *concejo* from these simple beginnings to the early fourteenth century is the subject of this book. A disciple and collaborator of Don Claudio Sánchez Albornoz at the University of Buenos Aires, Miss Carlé has already made significant contributions to the study of medieval Castilian economy.

This well-documented volume discusses the origins of the *concejo*, its transition from rural to urban settlement, its governmental and social structure, the relations between the urban and rural settlers in the municipal district as well as relations among Christian, Muslim, and Jewish inhabitants, economic life, and the role of the *concejo* in the public affairs of the realm. Tracing the history of municipal liberties, Carlé indicates a slow upward trend rapidly accelerating in the middle of the twelfth century when the Crown extended broad rights of self-government to the towns. The assembly of citizens ordinarily had the right to elect those officials and magistrates who would manage the business of the municipality. In the late thirteenth century the Crown began to take steps to recover greater control over municipal government by appointing town officials. This process reached its culmination in the reign of Alfonso XI; from then on the graph of municipal liberties shows a downward trend. In discussing the representation of towns in the Cortes, Carlé generally follows the lead of Sánchez Albornoz. It is particularly noteworthy that she questions Piskorski's assumption that the townsmen summoned to the Cortes came primarily as representatives of a social class, namely, the third estate. On the contrary, she argues with considerable merit that they were present on behalf of the *concejo* as an administrative district immune from the jurisdiction of ordinary royal officials. Fuller treatment of the economic activities of the towns and the addition of maps would have been helpful, but, on the whole, this book is a valuable and welcome synthesis of information based upon extensive use of the *fueros* and other sources.

Fordham University

JOSEPH F. O'CALLAGHAN

KING ALFRED & BOETHIUS: AN ANALYSIS OF THE OLD ENGLISH VERSION OF THE *CONSOLATION OF PHILOSOPHY*. By F. Anne Payne. (Madison: University of Wisconsin Press. 1968. Pp. viii, 151. \$6.50.)

THAT the early medieval Germanic world fits only awkwardly in a Roman frame is proved again by F. Anne Payne. King Alfred, like Boethius, was concerned with "certain elementary problems of human existence," but his selection of Boethius' *Consolation of Philosophy* to "translate" forced him to adjust either the Boethian text or his answers to Boethian problems. His attempt at loyalty to both created a work—the Old English version of the *Consolation*—which the author is the first to analyze thoroughly for its concepts, unity, and relationship to its model.

Her conceptual analysis examines primarily four major ideas. "Alfred's Rejection of Boethian Order" leads the King to omit Boethius' "reconciliation of foresight and free will" and to reject Boethian concepts of inevitability and eternity. "Alfred's Idea of Freedom," which he substitutes for Boethius' order, emphasizes freedom to act (whence evil stems) instead of Boethian freedom to think (with evil a result of intellectual misunderstanding), and the interplay of divine and human time, wisdom, and freedom instead of Boethius' providential, deterministic order. "Wyrd, Fortuna, and Fatum"—a problem Ladislaus Mittner's uncited *Wurd* would have illuminated—displays Alfred's linguistic escape from Boethian categories, with the ambiguities of "wyrd," "the partial translation of 'fortuna,'" and "the exclusive translation of 'fatum,'" delivering him from Boethian rigidities of fated order. "The Figures of Wisdom and Philosophy" shows Alfred's substitution of the less limited Wisdom, particularly associated with "the mind, God, and kingship," for the more academic Philosophy, a shift as significant for Alfred's royal role as his substitution of Mod, the king beset by problems, for Boethius as the dialogue's other figure.

Payne concludes that the flaw of the Old English version "lies in a discrepancy between thought and style." On the one hand, Alfred sticks closely to Boethius' form, although substituting a less formally logical discourse; on the other, he moves far from Boethius' world view. The problem is not so much the philosophical or linguistic inadequacies of Alfred, on which earlier criticism has focused, as the placing of his concepts and assumptions in an inappropriate framework. Underlying all is Alfred's awareness of a ruler's problems: the role of action, freedom to accomplish something despite human limitation, heroism in the face of "wyrd." Alfred was not Boethius, but Payne has shown that it is enough that he was Alfred.

Lawrence University

WILLIAM A. CHANEY

IMPERIAL CONSTANTINOPLE. By Dean A. Miller. [New Dimensions in History: Historical Cities.] (New York: John Wiley and Sons. 1969. Pp. ix, 226. Cloth \$8.85, paper \$4.95.)

THIS overpriced little book in a series (edited by Norman F. Cantor) that would seem to be paralleling the University of Oklahoma's "Centers of Civilization" series will certainly excite some comment among Byzantinists for the unusual, if not curious, way in which Constantinople and Byzantine civilization are dealt with. Cluttered with solecisms of various kinds, this book attempts to apply the terminology and techniques of modern urbanology to the Byzantine capital in the tenth century A.D. The tenth century is chosen because the author has "tried to describe Constantinople at the height of its powers: not the tragic, embattled remnant of tattered glories, but the living

New Rome of its best years, when it was the exemplification of an important unitary theory of politics, spirit, and secular life—an imperial city reflecting an imperial theory.” Miller sees Constantinople as the prototype of the imperial capital, a capital that iconically and symbolically represents the Empire and the emperor but at the same time is a planned city that traces its genesis back to the rationalized city-type of the Hellenistic period and the older “cosmogenic and symbolic” cities of the ancient Near East. The king-emperor plays the important role of city creator, and this is reflected in the planning and structure of the city itself. For Miller, the Byzantines had a predilection for the symbolic relationship that Lewis Mumford describes as “materialization,” the solidification of an idea in material.

Concentrating on the city, the economy of the city, the bureaucracy, the common life, and the aristocracy, Miller strives to provide the reader with a comprehensive view of the dynamics of an urban society within an imperial civilization and proclaims that Constantinople was the ritual center of the Empire and that Byzantine civilization is so defined. The analysis provided in this book tends to ignore what Byzantium really was—a Greek Orthodox state in which religion played a dominant role. Miller hardly mentions Eastern Christianity and devotes no chapter to the role of Orthodoxy or the Patriarchate in the Byzantine Empire and in the capital; he thus tends to distort an already highly artificial analysis of Constantinople.

Dean Miller warns his readers against a fragmented historical reconstruction and unfair re-creation of Byzantium. With an abundance of contemporary social science jargon, he has given the reader some new insights but essentially an incomplete view of Constantinople that few will recognize and some will reject outright.

Colgate University

JOHN E. REXINE

THE NORMANS AND THE NORMAN CONQUEST. By *R. Allen Brown*. (New York: Thomas Y. Crowell Company. 1969. Pp. viii, 292. Cloth \$5.50, paper \$3.25.)

A SUPERFLUITY of books, most of them jejune, many downright silly, marred the nine-hundredth anniversary of the Norman Conquest. R. Allen Brown is fortunate to have missed that glittering historical carnival, for his superb book would likely have been ignored through guilt by association.

Intended primarily for intelligent students and for the “elusive general reader,” this work forthrightly takes a “strong line in stressing the importance of the Norman Conquest.” Brown’s synthesis is well written, logically organized, solidly grounded both in the sources and in the secondary literature, and learned, yet it makes no claim to present new material. His book tells of England and of Normandy before the Conquest, of the relations between them in the eleventh century, of the Conquest, and of England under the severe rule of its first Norman king. English society and civilization of the eleventh century emerge from the author’s pen shorn of qualities not worth preserving: gone are the exaggerated glories of the Anglo-Saxon chancery, central administration, unity, “feudalism,” culture, and the Church. While not wholly original in substance, the strength and persuasiveness of his treatment of the crisis of 1051 and ensuing events are so convincing as to constitute an original contribution: Brown argues that it was precipitated by the Confessor’s promise of the succession to Duke William, and by Godwin’s reaction to the gossamer king’s act. He also handles the mysterious matter of Earl Harold’s oath to William with great skill. His discussion of Norman England leaves no reasonable doubt that the Normans did indeed introduce knight service into England during the Conqueror’s reign. Brown’s ability in handling the evidence in these

instances is typical of this fine historian at his best: it is a pleasure to share his scholarly analysis of the sources, his cool and precise reasoning, and his urbane criticism of others' historical judgments. This work is excellent political history; it takes its place in the literature of the Norman Conquest as the strongest and most lucid contemporary statement of the cataclysmic school of interpretation.

University of Georgia

JAMES W. ALEXANDER

THE KING'S HALL WITHIN THE UNIVERSITY OF CAMBRIDGE IN THE LATER MIDDLE AGES. By *Alan B. Cobban*. [Cambridge Studies in Medieval Life and Thought, Third Series, Volume I.] (New York: Cambridge University Press. 1969. Pp. xvi, 355. \$11.50.)

BASED on an unrivaled series of medieval collegiate accounts—twenty-six volumes in all—this book introduces the “Third Series” of “Cambridge Studies in Medieval Life and Thought” in a manner worthy of the distinguished founder and reviver of those “Studies,” Dr. G. G. Coulton and Dom David Knowles. What is even more important, this work sets a model for future histories of other colleges at Oxford and Cambridge.

Dr. Cobban has done much more than all but one or two of his predecessors. He has put the history of a Cambridge college in the full context of English university history. He has therefore given his readers perspective on the developments of King's Hall and has also clarified much that was obscure in the history of medieval Cambridge itself. For instance, he has shown that King's Hall, little noticed except as one of the units combined in the foundation of Trinity College, had influence on both academic and institutional developments at Cambridge and probably at Oxford. King's Hall, an adjunct of the Chapel Royal, was a part of the king's household in Cambridge, a source of educated servants for the Crown, and thus a stimulus and an exemplar for other collegiate foundations. He has also provided a more balanced view of the reputation of medieval Cambridge than that deriving from the great work of Hastings Rashdall.

Among the more interesting parts of Cobban's study are the chapter in which he examines the role King's Hall had in the development of tutorial instruction and collegiate lectures, the chapter in which he describes collegiate life in King's Hall and brings light to such shadowy persons as pensioners, commoners, and semicommoners, and the one in which he traces the careers of fellows of the college. On the other hand, the plates showing excerpts from the King's Hall accounts add nothing to the book and might have been replaced by one or two of the lists of fellows that he omitted and deposited in Trinity College Library, Cambridge.

Scripps College

MARK H. CURTIS

THE SCHOOL OF PETER ABELARD: THE INFLUENCE OF ABELARD'S THOUGHT IN THE EARLY SCHOLASTIC PERIOD. By *D. E. Luscombe*. [Cambridge Studies in Medieval Life and Thought, New Series, Volume XIV.] (New York: Cambridge University Press. 1969. Pp. xiii, 360. \$12.50.)

ONE of the more fruitful approaches to the Scholastic theologians of the twelfth century has been to view them in terms of schools. As the century progressed, theologians increasingly worked in close cooperation with their students and wrote their treatises in the form of *reportationes*, which were student versions of the teacher's instruction. By the end of the century it becomes at times impossible to distinguish the master from

the school. Dr. Luscombe has applied this approach to the most celebrated and controversial theologian of the twelfth century, Peter Abelard. To study Abelard in the context of his school, he has collected all contemporary evidence about the numbers and names of his students, traced the transmission and dispersal of his manuscripts, and studied his distinctive doctrines. Luscombe is a historian who takes theology seriously. Rather than dealing with vague matters such as methodology, or broad issues such as faith and reason, Luscombe identified those theological propositions that distinguished Abelard from his contemporaries. Following this doctrinal approach elaborated by Artur M. Landgraf, he traces the fortune of these specific theological notions in Abelard's direct followers and in his critics such as the schools of Laon and Saint-Victor in Paris, Gratian, Peter the Lombard, and Robert of Melun.

Now that Abelard's circle has been well illuminated by this doctrinal approach, we can compare it with the rival schools of the twelfth century. Although written and rewritten in many versions, his writings give no evidence of being composed as *reportationes* in his school. The manuscript evidence is comparatively small. Less than eighty manuscripts survive for all of his treatises, some of which exist in only one copy. Although twenty-one disciples can be named, only a handful of treatises remains from direct students, and most of these are by anonymous or obscure authors. By the end of the century there is no one who can be associated with Abelard's school. His salient theological doctrines were studied and modified by his critics, notably the school of Saint-Victor, and it was they, not his followers, who were responsible for transmitting his influence. Compared with Gilbert de la Porrée, Peter the Lombard, and even Hugh of Saint-Victor, whose followers were numerous and prominent throughout the twelfth century, Abelard was unsuccessful in establishing a durable school of theology. His failure may be attributed to his own idiosyncrasies and the crushing blows of two ecclesiastical condemnations. His principal contribution, therefore, was of a solitary and personal nature: to provoke other thinkers to clarify the main problems of theology. Seen in the light of Luscombe's excellent study, Abelard was the gadfly, the Socrates of twelfth-century theology, but not the founder of an influential school.

Johns Hopkins University

JOHN W. BALDWIN

BRONNEN VOOR DE ECONOMISCHE GESCHIEDENIS VAN HET BENE-
DEN-MAASGEBIED. Volume I, 1104-1399. Edited by J. F. Niermeijer. [Rijks
Geschiedkundige Publicatiën, Major Series, Number 127.] (The Hague: Martinus
Nijhoff. 1968. Pp. xxvi, 832.)

THE late J. F. Niermeijer, whose scholarly achievements were recently honored by a magnificent *Festschrift*, was an indefatigable worker. Certainly the leading Dutch medievalist at the time of his death in 1965, Niermeijer was known to all medievalists because of his *Mediae Latinitatis Lexicon Minus*. But what many medievalists did not associate with Niermeijer was his fine research on the economic history of the Low Countries. This present volume, which is a culmination of archival work with economic records during thirty years, demonstrates Niermeijer's incomparable knowledge of the Dutch archives.

Concerned only with the economic history of the region around the lower Meuse River, Niermeijer, before his death, had collected and edited all the documents—charters, acts, memorandums, and toll records—pertaining to this region centered about the port of Dordrecht. This collection, as now published, was completed under the direction of T. S. Jansma with the assistance of other Dutch medievalists. The first part

contains 723 items, mostly charters and acts dating from 1104 to 1399. As in most such editions, Niermeijer has published *in extenso* all documents hitherto unedited, re-edited documents poorly edited by other scholars, made slight corrections of previously edited documents, called attention to new texts and other copies, and merely referred to documents already well edited and requiring no corrections. In each case he has provided a brief synopsis of the document and, when needed, a critical apparatus. The second part consists of records of tolls collected at Dordrecht (1380–1385), Heusden (1378–1380, 1391–1393), Geervliet (circa 1370), and Strienemonde (1388–1389).

Although H. S. Lucas and J. de Sturler have pointed out the rise of Dordrecht as a leading port during the thirteenth century, this collection of documents will enable scholars to do for Dordrecht what H. J. Smit has done for the counties of Holland and Zeeland, W. S. Unger for Middelburg, and R. Doehaerd for Antwerp. This volume will also enable R. Hâpke's basic study on the rise of the Dutch economy to be revised. Brief examination of the records in this book suggests that intensive study of the lower Meuse region will show that Dordrecht, with its superb location on the estuary of the Meuse, arose and thrived on the economic resources of areas located hundreds of miles away.

While Dordrecht never achieved the economic domination of such ports as Bruges, Antwerp, and Amsterdam, it became at times the principal entrepôt for shipments of English wool and an international port frequented by merchants and ships from all parts of Europe. It should be emphasized that the counts of Holland advantageously used Dordrecht and the Meuse when the English kings, during their frequent conflicts with the kings of France and the counts of Flanders, needed other than Flemish outlets for their wool. This skillful use of these resources largely explains the remarkable economic spurt of the county of Holland during the thirteenth century, a spurt that marked the beginning of a shift northward in commerce from Bruges to Antwerp and then to Amsterdam. Until the thirteenth century, however, the economy of the lower Meuse region and of all Holland was pathetically behind that of such neighbors as Flanders, Brabant, Liège, and Namur. As these documents emphasize, it is difficult to find evidence of commerce and industry in the county of Holland before the twelfth century.

A model of what such a collection of documents ought to be, this treasure house will be eagerly used by all historians concerned with medieval social and economic history.

Brown University

BRYCE LYON

LE SALARIAT DANS L'ARTISANAT PARISIEN AUX XIII^e–XV^e SIÈCLES: ÉTUDE SUR LE MARCHÉ DE LA MAIN-D'ŒUVRE AU MOYEN ÂGE. By Bronislaw Geremek. Translated from the Polish by Anna Posner and Christiane Klapisch-Zuber. [École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Industrie et artisanat, Number 5.] (Paris: Mouton & Co. 1968. Pp. 147. 22 fr.)

FIRST published in Polish in 1962, this book uses late medieval Paris as a case study to reopen the question of how far skilled and semiskilled labor in urban industry was being pushed into the status of a proletariat during that period. The translation is most welcome because Geremek supplemented the several printed editions of Paris guild statutes with archival research. He is critical of De Lespinasse's edition of the post-1300 statutes in the *Histoire générale de Paris* for leaving out their informative preambles. Other gleaning yielded stray reports of litigation that are still more informative

regarding practices that varied from guild rules. Geremek argues suavely that the problem of urban proletarianization was still quite marginal in the fifteenth century.

Although he is well aware that Paris was hardly a typical city, he generalizes his conclusions by concentrating on the two largest industries: textiles and building, both of which had many wage earners. But these groups were too amorphous and contained too many different categories of workers to form a significant permanent proletariat. True, his information about unskilled men is slight and points, so far as it goes, to a pattern of floating about from one occupation to another and gravitating to one or another of the larger towns of a region between jobs. Evidence about the skilled men is better, but it gives no more consolation to those who believe the workers' conditions were ideal (if anyone still does) than to those who want to interpret every conflict as a class conflict. The book is refreshing in that it avoids polemic on the tired notion of medieval capitalism; it simply describes the urban economy as mercantile.

University of Michigan, Ann Arbor

SYLVIA L. THRUPP

THE BLACK DEATH. By *Philip Ziegler*. (New York: John Day Company. 1969. Pp. 319. \$6.95.)

IN recent decades the epidemic of plague that invaded Europe between 1348 and 1350 has been the subject of a considerable literature from the pens of medievalists and social historians. How did this epidemic affect the economic development of Europe, its demographic composition, and its emotional and intellectual outlook? Such questions form the major concern of Mr. Ziegler's attempt "to synthesize in a single readable but reasonably comprehensive volume the words of the contemporary chroniclers and the work of later historians." After a sketch of the origin of the epidemic, the nature of the disease, and the state of Europe at the time of its invasion, the author devotes most of the book to its progress and effects in Europe and, above all, in England. The last five chapters, based almost exclusively on English material, create an imaginative picture of the plague in a medieval village and discuss mortality, social and economic consequences, effects on education, agriculture, and architecture, and, finally, on the Church and the mind of man. Matters like medical knowledge and hygienic conditions of the time, flagellantism and persecution of the Jews as alleged propagators of the plague are skillfully woven into various chapters.

Though the author modestly describes himself as an amateur and disclaims virtually any original research, he shows an impressive acquaintance with old and recent publications and makes no concessions to vulgarization. He is very cautious in his inferences, and his conclusions are not startling. About one-third of the population is said to have died; it is conceded that the Black Death accelerated the commutation of feudal services into payments, that it led to a rise of wages, and that it contributed to the Peasants' Revolt of 1381 without being its sole cause. Frequent warnings against reliance on medieval statistical data, a tendency to avoid exaggerating or underestimating the effects of the Black Death on man's relationship to the Church and society, while yet conceding that it was "a catalytic element of the first order," render the book "sound," but also inject a note of repetitiousness.

Contemporary notions of infection and contagion are treated rather sketchily, and the position of the plague of 1348-1350 within the epidemiological context of subsequent outbreaks (seriously mentioned only on p. 234) ought to have received greater attention from the outset. Apart from the general historian who knows but little of the Black

Death, historians of medicine and of science who have paid little attention to the work of social historians will benefit most from this book.

Johns Hopkins University

OWSEI TEMKIN

Modern Europe

ACTION AND CONVICTION IN EARLY MODERN EUROPE: ESSAYS IN MEMORY OF E. H. HARBISON. *Theodore K. Rabb* and *Jerrold E. Seigel*, Editors. (Princeton, N. J.: Princeton University Press. 1969. Pp. xii, 463. \$13.50.)

THE respect and affection with which the late Mr. Harbison was regarded by his pupils, colleagues, and friends are clearly reflected in the brief preface to this book. The volume itself does him credit. Besides its proportion of dutiful but dull *Festschrifterei*, it contains a higher than usual number of useful or stimulating pieces. There are twenty essays, divided (on principles that, though clearly stated by the editors, seem somewhat arbitrary in practice) between "Faith, Reason and the World of Action" and "Christians, Scholars and the World of Thought." Reviewing such a composite work ideally means either listing all the contributions seriatim or picking out a few that seem especially rewarding. I follow a compromise course.

Among studies devoted to particular regions, British history has pride of place, and the word "British" must be stressed, for there are two studies of Scottish problems, of which one is most interesting: Maurice Lee's "Sir Richard Maitland of Lethington: A Christian Laird in the Age of Reformation." In this the laird's indifferent verses are cleverly scrutinized to show how peripheral for him (and for how many others?) the Reformation was, how little "reform" in fact manifested itself; J. Wilson Ferguson's "James V and the Scottish Church" goes some way toward explaining why this was so. As for English history, Lacey Baldwin Smith writes agreeably about Henry VIII's conscience and concludes that it was powerful as well as convenient, and A. J. Carlson analyzes Puritan pressure groups in the convocation of 1563, utilizing much new material. Other territories whose history is illustrated are France, with Joseph R. Strayer surveying the evolution of the concept of "the most christian king," and Davis Bitton discussing early seventeenth-century debates over the sale of offices, and Italy, with an account of the University of Florence, 1348-1434, by Gene A. Brucker, and a substantial analysis by Felix Gilbert of Gasparo Contarini's thought on religion and politics. From its place in the volume, one assumes that "Francis Bacon and the Reform of Society," by Theodore K. Rabb, belongs to the "World of Action." However that may be, it is a worthwhile reassessment of Bacon's position, stressing his fundamental conservatism in politics and gently correcting the work of some recent English writers.

When we turn to the studies of ideas, the prominence of Erasmus is hardly surprising. His views on government are touched on by J. Russell Major (also dealing with More, Seyssel, and Machiavelli). His influence in Spain is evident in Paul J. Hauben's essay on heresy in the peninsula. He figures in Richard M. Douglas' discussion of "talent and vocation in humanist and protestant thought" and in George Huntston Williams' discussion of "*salus extra ecclesiam* in Erasmus and the reformers." Also, he is one of the two heroes in Myron Gilmore's description of that last fierce and sad controversy between Erasmus and Alberto Pio of Carpi. Other individual thinkers cannot compete with Erasmus. More's *Utopia* is compared by J. H. Hexter with the Geneva of Calvin, and some of the same discomforts are discovered in each place. E. William Monter argues that the *Démonomanie* of Jean Bodin reflects its author's "shift of interest

from human history to natural history." I enjoyed reading "John Locke and the New Logic," by Wilbur Samuel Howell; it seemed to my inexperienced eye that Howell was saying something rather important.

Two of the remaining contributions are unlike their companion pieces. Lynn White, jr., discusses the iconography of *temperantia* and uses plates to illustrate his argument. It is a most interesting example of an artistic subject reflecting changes in its details as a result of new social or ideological pressures. In this case technology is shown gradually to dominate the concept with the introduction of the clock, the windmill, and other devices. Whether one can go on to regard as "non-verbal" evidence the fact that "natural power, mechanization . . . received the sanction of religious emotion and moral sensibility" seems to me somewhat more doubtful, and I shy at the use of the term "bourgeois ethos" even when "bourgeois" is in quotes. Lastly I think that Jerrold E. Seigel has written a stimulating and perceptive essay on "The Teaching of Argyropoulos and the Rhetoric of the First Humanists." It turns in part on Argyropoulos' preface to his translation of Aristotle's *De interpretatione et generatione ratiocinationis*, of which he gives a critical edition. In so far as this leads Seigel to wonder whether Ficino and company were "humanists," I think it is most useful. But I wonder if we should not move very cautiously in matters rhetorical. "Rhetorio" cannot fail to have a pejorative sense nowadays, I suppose, but we cannot avoid it. Some misguided and untaught notion of the art of persuasion lies behind my own halting words; other parallel notions may be discerned in the pages of all the contributors to this volume, including Seigel. What Bruni admired was to pass away in the fullness of time (several centuries). Howell, writing on Locke, talks of "the humanistic enterprise of transmitting ideas," but he also refers excitingly to Locke's influence in "the emerging new rhetoric." Elsewhere I notice that Bitton feels he is presenting "an instructive case study of political rhetoric in early modern Europe." I should like Seigel and Howell to ask Bitton what he means by this. I hope I may one day learn the answer.

University of Edinburgh

DENYS HAY

ERASMUS AND FISHER: THEIR CORRESPONDENCE, 1511-1524. By *Jean Rouschause*. [De Pétrarque à Descartes, Number 16.] (Paris: Librairie Philosophique J. Vrin. 1968. Pp. 108.)

THE emphasis in the prefatory matter and notes of this substantial little book is upon Fisher rather than Erasmus for, as Father Rouschause observes, "The life of the Bishop of Rochester, apart from his tragic fate, is not so well known as that of Erasmus." The translations of the eighteen letters comprising this corpus of correspondence (with the Latin text on facing pages) are, however, the real strength of the book: they are fresh, clear, and accurate, and a number of the letters appear in English for the first time. For some curious reason, the author has taken the Latin text from the Leiden edition of Erasmus' *Opera Omnia* rather than from the much more standard Allen, *Opus Epistolarum Des. Erasmi*. Fortunately there are few serious variations in the two texts. On the other hand, Rouschause acknowledges his debt to the Allen edition and is clearly dependent upon it at many points, including following the numbering. Yet, for five of the letters (Numbers 1, 2, 3, 6, and 17), he departs from the dates Allen assigns, and without any explanation. He might have chosen to comment upon his reasons for challenging the chronology, which is one of the most carefully documented and closely reasoned parts of Allen's extensive apparatus. On balance, however, this book should be very useful not only in making available in translation another portion of the important

Erasmus correspondence but also in helping to fill in the much too sketchy picture we have of his learned, kind, and noble-spirited friend.

Wichita State University

J. K. SOWARDS

ARMY ROYAL: HENRY VIII'S INVASION OF FRANCE 1513. By C. G. Cruickshank. (New York: Oxford University Press. 1969. Pp. 223. \$6.25.)

THE FIELD OF CLOTH OF GOLD: MEN AND MANNERS IN 1520. By Joycelyne G. Russell. (New York: Barnes and Noble. 1969. Pp. xiii, 248. \$6.50.)

It is appropriate that these two books be reviewed together, for they have much in common. Both were written by noted English historians focusing on a period of English history rich in pomp and pretense and correspondingly barren in concrete diplomatic or military achievement. More specifically, they are concerned with Anglo-French relations during the early period of Henry VIII's reign; each examines in vivid detail Henry's visionary attempts to deal with the French problem—once by war, once by dazzling diplomacy. The setting of each extravaganza is within a few miles of Calais. Both narratives are based on contemporary chronicles.

The campaign described by Mr. Cruickshank was unlike any other. It was as much a theatrical production as a war. The size, discipline, and equipping of the English army are impressive, but a royal wardrobe staff of 49, with 15 aids for the King's beds, 115 for his chapel, 276 cooks for the royal meals, and 500 pages, grooms, sewers, ushers, and so forth, is ridiculous even in the Renaissance. Henry took several wagonloads of clothes and supplemented these with lavish purchases along the way and, in addition to many tents—including the great tent of cloth of gold—carried with him a prefabricated timber house, complete with chimneys, fireplace, and furnishings.

Army Royal is a superlative book. The author's narrative of the march from Calais to Bomy and the ensuing siege of Théroutanne (usually referred to as the Battle of the Spurs, even though there was no battle fought) is skillfully written, with a master's touch for just the right amount of detail. Furthermore, his descriptions of troop movement, victuals, munitions supply, discipline, encampment, and other aspects of organization and recruitment are as poignantly handled here as they were in his earlier *Elizabeth's Army*. Cruickshank's thesis is that for all of its enormous expense and theatrics, the campaign of 1513 was without positive gain to Henry or to England. It revealed the King's rank incompetence in the art of war (along with some notable luck), as well as his overburdening egotism and obsession for show. Only Wolsey (temporarily) and the Emperor Maximilian profited from the campaign. By diverting the French army and neutralizing Tournai, Henry did Maximilian a valuable service—for which he also paid the Emperor twenty thousand pounds!

Dr. Russell's account of the summit meeting between Henry VIII and Francis I seven years later is less judicious in the use of detail but no less fascinating as a picture of royal life at its most flamboyant. The festivities that took place in June 1520 on "The Field of Cloth of Gold," between Guines, in the English pale, and Ardres, France, were so sumptuous that they almost defy description. Russell's integrated verbal panorama of the fourteen-day fete—with its assembly of over six thousand royalty and nobles, with all of their retinue and horses, frantically competing in feats of arms, displays of dress, and gastronomic marathons that make the imagination swirl—is indeed an achievement. Through all of the banquets and balls, the author follows the ubiquitous Cardinal Wolsey in his moment of glory, scurrying about making arrangements down to the last detail and concluding a meaningless treaty with the French King and an equally super-

fluous marriage alliance between Henry's four-year-old daughter, Mary, and the Dauphin of France. Like his war policy, Henry's (or Wolsey's) grandiose scheme for a diplomatic tour de force was short lived. But the memory of that unique and luxurious meeting has lived on and has now been given new life in this descriptive book.

Brigham Young University

DE LAMAR JENSEN

YEAR BOOK XIII. [Publications of the Leo Baeck Institute.] (London: East and West Library for the Institute. 1968. Pp. xxiii, 358.)

THIS latest volume in the series published by the Leo Baeck Institute maintains the high level of scholarship and interest set by its predecessors. Subtitled "Jews amidst Political Turmoil," it consists of twelve contributions loosely connected by the theme of the struggle for Jewish identity and survival in the German-speaking lands since the Reformation and especially in the nineteenth and twentieth centuries.

Though all the contributions are in their way interesting, some, like the late Eugen Mayer's notes on a short-lived ecumenical Protestant-Catholic-Jewish journal in Frankfurt in 1837, are in the nature of historical curiosities, or, like the comments of Robert Pois and Eduard Rosenbaum on the problem of Rathenau's Jewishness and Carl Cohen's essay on Bucer's attitude toward the Jews, deal with relatively marginal aspects of important figures. In this category also are the early diaries of the historian Heinrich Graetz, which, nevertheless, provide some interesting insight into the *shtetl* milieu that still prevailed in Prussian Poland in the first half of the nineteenth century.

Several contributions are of first-rate importance: for instance, Hans Liebeschütz' luminous pages on Hermann Cohen, whose dream of Kantian synthesis between German and Jewish values was already fading by the time of his death in 1918. Extremely interesting is the detailed discussion by Jacob Toury of the abortive movement for Jewish communal unity from 1893 to 1920, a movement reflecting the struggle between the official liberalism to which the majority of German Jews still adhered and newer, more defiantly Jewish-nationalist ideologies like Zionism. The radicalizing impact of World War I, the encounter with the East European Jewish masses in the occupied areas, the Balfour Declaration, and, finally, the German Revolution make a fascinating counterpoint with movements in German society as a whole.

The central and yet precarious position of the Jews in the Weimar Republic is reflected in what is perhaps the most significant selection in the book—lengthy excerpts from the diaries of the journalist Ernst Feder, a leading member of the *Berliner Tageblatt* and of the Democratic party. Ably edited and annotated by Arnold Paucker and with apparent emphasis on their Jewish content, the diaries vividly record the impressions of a man immersed in the literary and political life of his day. The excerpts cover only the years 1930–1932, and cause one to anticipate the publication of the full diaries in 1970.

Though anti-Semitism is, of course, a thread running through the entire volume, it is dealt with specifically in two grim selections. The first, by the Czech scholar František Červinka, re-creates with remarkable objectivity the disgraceful Hilsner ritual murder trial of 1899. The second, by S. Adler-Rudel, is about the notoriously ineffectual Evian Conference of 1938 on German-Jewish refugees and includes, as Adler-Rudel aptly calls it, the "subdued but desperate" text of a memorandum to the conference by the *Reichsvertretung der Juden in Deutschland* proposing measures to facilitate emigration.

Finally, there is a very useful and complete bibliography at the end of the volume; it lists books on German-Jewish history and related subjects for the year 1967. Their number and variety testify to the continued interest in the historic importance and vitality of this now-vanished community.

Washington University

SOLON BEINFELD

IL PENSIERO POLITICO DI HALLER E ROSMINI. By *Mario Sancipriano*. [Centro di Ricerca del C.N.R., Istituto di Filosofia, Facoltà di Magistero, Università di Genova. Studi sul pensiero filosofico e religioso dei secoli XIX e XX, Number 11.] (Milan: Marzorati Editore. 1968. Pp. 338. L. 3,400.)

SANCIPRIANO's purpose in studying the political ideas of Carl Ludwig von Haller and Antonio Rosmini-Serbati is to achieve an understanding of some of the political complexities of our own time and to evaluate more objectively the political thought of the Restoration. In Haller and Rosmini he identifies some of the positive aspects of conservative, even reactionary, thinking that stimulated the development of nonrevolutionary aspects of European thought in the nineteenth century.

He shows how Haller, in spite of his "patrimonial" concept of the state, still offers thoughts for meditation on the nature of political power and on the relationships between sovereignty and independence and how Rosmini contributed to the development of liberal Catholicism and modern constitutionalism.

The volume is composed of two monographs of approximately equal length: the first devoted to the Swiss Haller, the second to the Italian Rosmini. In each the author painstakingly sets forth the political theories of one of the two thinkers, discussing the similarities and differences with the other lucidly and incisively. Sancipriano's originality rests on the "confrontation" between the thinking of the two theorists. He shows that Haller was more specifically a political scientist who endeavored, though largely unsuccessfully, to make politics a science and also an art directed to the preservation of the state, the "legitimate" state, whereas Rosmini, a philosopher and a priest, was concerned with the metaphysical order as a source of morality and thus of human rights. Rosmini, influenced at first by the older theorist, eventually accepted a measure of liberalism and constitutionalism and recognized and even advocated the necessity of social, political, and religious reforms.

Sancipriano is a scholar who possesses a sure and vast knowledge of the period he is exploring as well as a vital interest in current basic political questions. Besides having published several studies in the field, he is engaged in translating into Italian Haller's six volumes on the *Restoration of Political Science*, the first volume of which was published in 1963. The present study contains a valuable twelve-page bibliography, and the four appendixes include Haller's hitherto unpublished *Éléments de droit public, de droit des gens et de la haute politique*.

Wellesley College

GRAZIA AVITABILE

- LE RELAZIONI DIPLOMATICHE FRA L'AUSTRIA E IL GRANDUCATO DI TOSCANA. Third Series, 1848-1860. Volume III (10 MAGGIO 1851-30 DICEMBRE 1852). Edited by *Angelo Filipuzzi*. [Documenti per la Storia delle Relazioni Diplomatiche fra le Grandi Potenze Europee e gli Stati Italiani 1814-1860. Part 2, Documenti Esteri. Fonti per la Storia d'Italia.] (Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1968. Pp. 575. L. 5,000.)
- LE RELAZIONI DIPLOMATICHE FRA LA GRAN BRETAGNA E IL REGNO DI SARDEGNA. Third Series, 1848-1860. Volume IV (27 GENNAIO 1852-10 GENNAIO 1855). Edited by *Federico Curato*. [Documenti per la Storia delle Relazioni Diplomatiche fra le Grandi Potenze Europee e gli Stati Italiani 1814-1860. Part 2, Documenti Esteri. Fonti per la Storia d'Italia.] (Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1968. Pp. xv, 390. L. 5,000.)

BARON Karl Hügel, the Austrian minister at Florence, reported much that was trivial, but these documents do present a graphic picture of the unhappy problems of a weak state when placed under the influence of a large and reactionary power. The Constitution of 1848 was abrogated, and Tuscany joined Modena, Parma, and Rome in October 1851 in an agreement to work together to maintain order, security, and tranquillity. There was much unrest, even a murder attempt on Giovanni Baldasseroni, President of the Council of Ministers. Much of the volume deals with actual or imagined subversion, with arrests and condemnation. To Baron Hügel, Great Britain was the real villain and was overly zealous in protecting the rights of Englishmen who were caught up in the police net. Grand Duke Leopold II emerges as a weak man strongly under the influence of Rome and Vienna and certainly not a man to lead his country toward change and progress. The costs of supporting some ten thousand Austrian soldiers were too great for the budget; cuts had to be made in education and finally in the number of Austrian soldiers.

In marked contrast, the reports of Sir James Hudson, British minister to the Kingdom of Sardinia, reveal progress and vitality. Hudson had harsh words for Austria for confiscating the property of refugees who had come to Piedmont and for threatening to crush Piedmont as the last barrier to Austria's absolute rule in Italy. France enjoyed no real trust, but Austrian pressure was pushing Piedmont toward alliance with France. Britain sought to settle the refugee question and to calm Austria's fears with regard to unrest in Italy so that Austria could enter the war against Russia. A part of this game was to persuade Piedmont to send forces to the Crimea. Russia, meanwhile, was fomenting unrest in Italy. Cavour seized the invitation to participate in the Crimean War, and the matter was settled by January 10, 1855, even though the terms offered to him were none too favorable. Hudson had harsh words for Mazzini and worked to prevent Garibaldi from visiting Genoa. Britain worked to settle a territorial dispute between Piedmont and the Prince of Monaco, in part because of Foreign Secretary Lord Clarendon's fear that otherwise the Prince might sell his property to the United States, a move that Clarendon was prepared to oppose.

In addition to documents in the Public Record Office, the editor has used the Cowley, Russell, and Clarendon Papers and those of Queen Victoria. Many of the documents in this volume were published in his *Le relazioni diplomatiche tra la Gran Bretagna ed il Regno di Sardegna, 1852-1856: Il carteggio diplomatico di Sir James Hudson* (2 vols., 1956).

Colgate University

WILLIAM C. ASKEW

LA QUESTIONE ITALIANA DALLE ANNESSIONI AL REGNO D'ITALIA NEI RAPPORTI FRA LA FRANCIA E L'EUROPA. Third Series, 1848-1860. Volume I-IV. Edited by *Armando Saitta*. [Documenti per la Storia delle Relazioni Diplomatiche fra le Grandi Potenze Europee e gli Stati Italiani 1814-1860. Part 3, Rapporti tra Stati Europei. Fonti per la Storia d'Italia.](Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1968. Pp. xi, 427; 419; 436; 432. L. 5,000 each.)

Of all the volumes in the collection of "Fonti per la Storia d'Italia" the ten volumes of Part III ("Rapporti tra Stati Europei") are the most significant and the most far-ranging because they cover the actions and policies of the Great Powers in relation to the successive Italian questions and crises. And, among these ten volumes, these four are the most important because they carry the documentation through the Sardinian annexations of central Italy, the Garibaldian expedition, the annexation of the Mezzogiorno, and the early recognitions of the new kingdom of Italy, covering the twenty-two months from September 1859 to June 1861. Since France was the hub of the diplomacy of this period, it is through the pages of the French correspondence with the Great Powers that we see displayed their policies and their interrelations. Even two smaller states are included: Spain, for the questions of Naples and Rome, and Switzerland, for the annexation of Savoy.

The quantitative proportions of this subseries are staggering: the texts of 1,120 documents are reproduced in a total of 2,715 pages. There are 136 pages containing just a chronological listing of the documents with summaries and, at the end, 30 pages of index. The reader has the best of both approaches by the chronological list and by a topical arrangement within the volumes: Volume I presents French circulars and correspondence with France's Vienna embassy; Volume II contains French correspondence with London, Berlin, and Frankfurt; Volume III, with Berlin, Frankfurt, and St. Petersburg; and Volume IV, with St. Petersburg and Madrid. My two disappointments are: the editor, unlike those of the *DDI*, does not discuss the unique collections among his sources, such as the Persigny and Thouvenel private papers, appearing for the first time in extensive reproduction; and, he omitted the use of the Walewski private papers in the Foreign Ministry Archives and the Thouvenel family papers in the Bibliothèque Nationale. In general, the quality of the editing is, as usual for these series, superb. Judicious selection, careful reproduction, and meticulous and helpful notes and cross references exhibit the excellence that we have come to expect from the works of Professor Saitta.

The usefulness of this subseries to historians can be inferred by the questions it covers: annexations of the duchies, Romagna, and Tuscany; the "reunion" of Savoy and Nice; the Garibaldi expedition; Umbria and the Marches and the Roman question; the Franco-Sardinian diplomatic break; and, finally, the unification and recognition of Italy. For biographies, there is much material for studies of Napoleon III, Alexander II, Francis II of Naples, Gorchakov, Rechberg, Schleinitz, Russell, Palmerston, Cavour, Persigny, and Thouvenel. The French Foreign Minister, Thouvenel, was famous for the cogency and clarity of his state papers, and in this compilation we find twenty-five of his circulars, among which are those on Savoy, the Chablais, the break with Sardinia, and the recognition of Italy. Likewise, his dispatch and memorandum for the Warsaw Interviews (Number 938) and Napoleon III's letter to the Tsar (Number 960) are notable inclusions. On the lighter side, we find the excitability of Persigny displayed in his letters to Thouvenel. The ambassador was delighted with Thouvenel's

clear and full instructions, and, casting reflections on Thouvenel's predecessor, Walewski, he remarked: "C'est un peu drôle de voir un agent féliciter son Ministre de lui envoyer des instructions, comme une chose tout à fait extraordinaire" (Number 265). But four months later the same ambassador snapped back at his same minister: "Puisque vous ne croyez pas avoir besoin de m'indiquer votre impression, permettez-moi de mon côté à ne pas vous parler de la mienne" (Number 419).

Historians in general, and distant American historians in particular, will be extremely grateful to Saitta and to the *Istituto* for this excellent continent-wide compilation of state papers on such a crucial period.

University of Pennsylvania

LYNN M. CASE

LES RELATIONS MILITAIRES FRANCO-BELGES: DE MARS 1936 AU 10 MAI 1940: TRAVAUX D'UN COLLOQUE D'HISTORIENS BELGES ET FRANÇAIS. (Paris: Éditions du Centre National de la Recherche Scientifique. 1968. Pp. 198. 20 fr.)

THIS interesting and compact monograph is the result of a joint study by a group of Belgian and French historians who have examined in depth Franco-Belgian military relations between March 1936 and May 10, 1940. The study is divided into two parts; the first covers the period to September 1, 1939, and the second, the balance of time to the eve of the invasion of Belgium by the Germans. Both parts are amply documented in specific sections. The two parts are linked by an introduction written by Pierre Renouvin, renowned French *doyen* of diplomatic history, and by Professor Jacques Willequet of the Free University in Brussels.

Much of the comment of these two authorities and of the joint study group centers on the basic issue in Franco-Belgian military relations before World War II: Belgian unwillingness to enter into a new military alliance after having withdrawn from the 1920 military convention in 1936. After King Leopold III made this announcement in his speech of October 14, 1936, the French security system of the Versailles and Locarno Treaties disintegrated. Small wonder that the French military and political chiefs showed much concern and kept pressing the Belgians for staff talks and other measures to restore cooperation.

After Belgium formally disengaged itself from its alliance obligations, its government maintained that the nation would defend its borders against any aggressor. Informal and low-level contacts between the French and Belgian military authorities continued, but the Belgian government repeatedly refused any substantial talks on the grounds that these could only take place after aggression had actually occurred. The French chief of staff, General Gamelin, appealed repeatedly for reconsideration but to no avail. General Van Overstraeten, who was the most influential military personage in the kingdom as the personal adviser to the King, did not deny that in case of German aggression the situation would be undesirable, but concluded all the same that no changes in the official position of the government would be possible until after an invasion. With an amazing naïveté, if nothing worse, the Belgian government expressed the opinion that a German violation of the frontiers of Belgium was "little likely" even though intelligence provided contrasting evidence.

Twice, in November 1939 and in January 1940, Belgian ministers, including P.-H. Spaak, urged preparatory talks, but, once the immediate danger of invasion had passed,

such talks were delayed. When the storm finally broke on May 10, 1940, it proved too late for any long-range preventive planning, and the debacle was at hand.

It would have been desirable to include materials pertaining to the eighteen days in May before the collapse of the Belgian Army. A real weakness in the copious documentation is the fact that they are not in date order and there is no index. Otherwise, they provide interesting information in a subject area, hitherto not well known and yet of much significance in the political-strategic antecedents of World War II.

American University

GUNTHER EYCK

CHARACTER AND STYLE IN ENGLISH POLITICS. By J. H. Grainger. (New York: Cambridge University Press. 1969. Pp. viii, 291. \$8.50.)

THE POWER OF PARLIAMENT. By Ronald Butt. Foreword by Karl Loewenstein. (New York: Walker and Company. 1968. Pp. xxxiv, 468. \$8.50.)

BOTH of these books are difficult to place. Mr. Grainger has written one of those discursive books, largely based on standard political biographies, that sounds as though it were written for the old BBC Third Programme, when the BBC really took itself very seriously. It is a pleasant book to read, full of miscellaneous information; in short, it is just the sort of book that one likes to have on one's shelves ready for a rainy day. But it breaks no new ground, and it has no very distinctive point of view. The theme is simply *Plus ça change, plus c'est la même chose*. However far we go back into the past, Grainger seems to say, we seem to stumble on the same type of person and the same type of activity.

Mr. Butt's book is provided on the wrapper with a subtitle: "An Evolutionary Study of the House of Commons in British Politics." But it is not a history in the ordinary sense. There is a longish retrospective section running to 127 pages and covering the history of the House of Commons from the beginning to 1931, which says nothing that has not been said before. Then there is a series of rather discursive chapters on 1931 and after, the object of which seems to be to show that the House of Commons is not such a bad old work horse as is sometimes made out, but that it needs better working conditions. Much of the text is devoted to past discussion of possible reforms, but there are also long sections on back bench opinion and the role of the opposition. The trouble about the book is that it does not quite seem to come into focus. Butt would argue that his main point is that "Britain is still a monarchy in the practice of government . . . in the sense that the power of the Executive is of a monarchical kind. The Cabinet is the monarchy in commission, with the Prime Minister as First Commissioner." But though this is a good point, and one worth making against those who argue that the cabinet is to a large degree a committee of the Commons, it is not made early enough in the book to shape its structure. As a result, the book is rather formless, and much the best of it is in two chapters of fairly snappy conclusions at the back.

Harvard University

H. J. HANHAM

THE CHARITY OF RICHARD WHITTINGTON: A HISTORY OF THE TRUST ADMINISTERED BY THE MERCERS' COMPANY, 1424-1966. By *Jean Imray*. ([London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1968. Pp. viii, 138. \$5.95.)

THE CURRIERS AND THE CITY OF LONDON: A HISTORY OF THE WORSHIPFUL COMPANY OF CURRIERS. By *Edward Mayer*. ([London:] Worshipful Company of Curriers. 1968. Pp. ix, 212. 3 gs.)

A HISTORY OF THE CARPENTERS COMPANY. By *B. W. E. Alford* and *T. C. Barker*. ([Hamden, Conn.:] Archon Books. 1969. Pp. 271. \$9.50.)

THE Whittington charities administered by the Mercers' Company (neither the sum of Whittington's benefactions nor the most famous of the Mercers' charitable trusts) consisted of a college of five priests attached to the parish church of St. Michael Pater-noster, London, which Whittington rebuilt before his death and where he was buried, and almshouses (confusingly called Whittington College) adjacent to the same church. The College of Priests was dissolved in 1548, but Whittington College continues, after an earlier move in 1824, with new pensioners' houses at Felbridge in Surrey. In her first two chapters, Miss Imray, archivist to the Company of Mercers, skillfully takes the reader through the intricate history of the colleges; a brief chapter that deals with the college of priests and three chapters on the almshouses complete this thorough and painstaking book.

In the last twenty-five years at least twenty-one histories of London livery companies, including these two histories of the Curriers and the Carpenters, have been published. Little need be said of the former. Mr. Mayer, a past master of the company, has written an engaging domestic chronicle larded with accounts of civic customs, pageantry, and public affairs and spiced with curious and amusing anecdotes that will give pleasure to many of his fellow Curriers. The history of the Carpenters' Company by Dr. Alford and Professor Barker, although commissioned by the company, is aimed at a readership of historians. The book contains an intelligently organized and well-written history of 170 pages followed by 60 pages of appendixes (including lists of honorary members, of masters and wardens, of clerks and beadles, a history of the company plate and jewels by Charles Oman, and a history of the hall's furnishings by John L. Nevinson), 32 pages of notes (many faulty and inadequate), and an index (inadequate). The authors give considerable attention to craft regulation and stresses within the company in the years up to about 1700 and to the company's finances throughout its history. They largely disregard civic, ceremonial, and friendly society functions of the company. The nineteenth and twentieth centuries, aside from finance, receive as light treatment as in most other company histories. In a salutary account of the company's early acquisition of property, the authors warn us of the pitfall in taking at face value bequests of land by will to corporate bodies. They are right, I suspect, in their view that the testamentary devise as a legal procedure for securing title "may account in part for the apparent rise in philanthropy . . . described by W. K. Jordan." (Imray does not discuss this procedure at length, but she does provide us with a more elegantly documented case of it in operation than those afforded by the Carpenters' records.) A number of slips and errors suggest, however, that Alford and Barker may be less at home in the pre-1700 period than in the later centuries. We are informed, for example, that the first property acquired by the Carpenters' Company in 1429 was held by leading members as trustees "in order to avoid the law of mortmain." Surely the real reason was that the company was not a corporation and did not receive its first charter of incorporation until 1477.

Plate 2 is described as a page from the Court Book (1604) "showing the signatures and timber marks" of the master and wardens. What it actually shows is that the master and wardens could not write their own names, which are all in the company clerk's hand, and that their painfully drawn marks are not necessarily timber marks. The Great Fire of London, we are told, ravaged an area bounded on the west by Tyburn. This would probably more than double its actual area. Errors of this sort are not the book's most serious defect. A history of the Carpenters by two economic historians could have made a valuable contribution, particularly as the records of the Carpenters are, for a lesser company, so relatively complete, but this work shows such signs of having been hastily and carelessly researched that its value is impaired. Although the authors claim to have given special attention to "developments in the craft," their discussions only draw together scattered references from a handful of published works on the building trades. They have not done any research (in records of litigation, for example) that would enable them to say anything new on the subject. They do not even give reliable guidance on such basic matters as size of company membership or numbers of apprentices. They have calculated the number of freemen from quarterage payments: "about forty" in the late 1430's, rising to "nearly fifty" in the mid-fifties, then continuing "at about the same level, with a tendency to fall, until 1477," and so on. Following what I take to be their procedures, I calculate 64 members at Michaelmas of the accounting year 1455, 32 at Easter, and 84 at Michaelmas 1456; 7, 14, 16, and 21 for the four quarters of 1469; 86, 55, 61, and 53 for 1470; 48, 40, 44, and 48 for the four quarters of 1489; 128, 133, 124, and 127 for 1490. Either they have not troubled to calculate the membership term by term or they have failed to explain these astonishing anomalies, which throw the whole procedure into question. Concerning apprentices, they state that before separate apprenticeship registers begin in the mid-seventeenth century one must get the numbers from the Court Books, which record apprenticeship bindings from 1573. The fact is that while G. T. Eltringham's typescript notes on apprentices in the Guildhall Library begin with the published Court Books of 1573-1593, the minuting of presentations begins September 26, 1571, in the prior volume for 1533-1573. Moreover, anyone who has spent some time with the company's records will see that the number of presentations can be got at irregularly until, and regularly after, 1486 through fees recorded in the wardens' accounts. A carelessness that is not infrequent can be seen in their analysis of the effectiveness of craft regulation. The authors state that in 1600 "the gap between the estimated number of carpenters in London and the number under the control of the Company is so large as . . . to suggest the view that only a minority of those working at the craft were doing so legally." The supporting argument, every step of which is questionable, depends first upon the percentage of carpenters in London's male labor force. This is assumed to have been 2.3 per cent because 2.3 per cent of the male labor force in Gloucester, Tewkesbury, and Cirencester "was employed as carpenters." A look at the table in the 1934 article by A. J. and R. H. Tawney from which the authors apparently got their figure of 2.3 per cent (they give no page reference) reveals that the percentage applies not to carpenters but to the "Building" industry including masons, tilers, glaziers, plasterers, paviors, and painters, all of whom had their own companies in London in the seventeenth century. The Tawneys do not even classify carpenters under "Building." They are put under the "Woodwork" industry, and there is no way of calculating from the Tawneys' figures the exact percentage of them in those three towns. The point is not that the authors' generalization about ineffective regulation is wrong—it conforms to the accepted view—but that the elaborate foundation to which it pretends is flimsy. There are numer-

ous instances in the pre-1700 portion of the book of sophisticated analysis set on ramshackle evidence.

University of Oregon

ROBERT G. LANG

THE ENGLISH PARISH CLERGY ON THE EVE OF THE REFORMATION.

By *Peter Heath*. [Studies in Social History.] (Toronto: University of Toronto Press. 1969. Pp. xiii, 249. \$6.50.)

THE historian who attempts a comprehensive analysis of the state of the English clergy on the eve of the Reformation is faced with a formidable task. His concern must be with the individual at the parish level where conditions differ widely from diocese to diocese and from parish to parish. The variables are many, and their combinations endless. Ideally such a study should follow a series of intensive studies of individual dioceses. Yet, except for the excellent *The Secular Clergy in Lincoln Diocese* of Margaret Bowker, no such studies exist. Hence Mr. Heath is reduced to the use of random evidence from a wide range and variety of sources, documentary and secondary. Such material does not lend itself to logical analysis and certainly not to statistical treatment. The exceedingly interesting cases he cites are too often unique. His conclusions, therefore, tend to be impressionistic, reflecting his own sound understanding of the pre-Reformation Church and illustrated and reinforced by the random evidence he has garnered.

The system of patronage, in which bishops and monastic foundations were as fully involved as were king and laity, was a principal obstacle to the recruitment of qualified parsons. Equally deleterious were the use of the better-trained clergy to staff the great offices of Church and state and the concomitant use of the well-endowed benefices to provide income for these offices. For the parish this meant absenteeism, pluralism, poverty, and a lowering of the quality of the men left responsible for the cure of souls. A significant though unplanned result in a number of cases, however, was the assumption of responsibility on the part of the parishioners in finding and providing for adequate ministers—training and precedent for the Church and dissent in the years to come.

Heath believes, however, that the great weakness of the Church in these crucial decades stemmed not so much from economic, social, or political causes as from the almost total lack of training of the parish clergy in the essentials of their pastoral work, intellectual as well as spiritual. They were woefully ill-equipped to meet the challenge of the New Learning and the Reformation. For this he holds the hierarchy strictly to blame. "It did not require a reforming monarch to appoint reforming bishops, it merely needed bishops who thought more deeply and carefully about the spiritual implications of their office." Where the leaders of the Church had no pastoral experience, no understanding of pastoral problems, these problems simply were not met. The Church in England was in fact an easier target for Henry's attack than the Rome of Clement VII.

Denison University

W. M. SOUTHGATE

THE STANHOPE OF CHEVENING: A FAMILY BIOGRAPHY. By *Aubrey Newman*. ([New York:] St. Martin's Press. 1969. Pp. 414. \$12.50.)

If a term from the physical sciences may be used to describe a literary and historical form, it would probably be correct to say that a "family biography" is a highly unstable compound. The author of such a work is constantly tempted to tarry over individuals—

the more interesting ones obviously—and thereby risks neglecting his larger purpose of tracing the family's fortunes over time. If he successfully avoids this hazard, he generally encounters another, the temptation to subordinate the family story—with its many original and distinct features—to the larger and better-known story of the political and social history of the nation. To maintain the integrity of the "family biography," as Aubrey Newman in his study of the Stanhopes has done, is no mean achievement.

Newman has written a book that is at once modest and useful. He has explored the family archives to good purpose; the social narrative is simple, but suggests how unchanging were certain aspects of aristocratic life in England from the seventeenth century to the twentieth. This is not to say that Newman has neglected the large political changes that occurred in the period and that had such importance for families like this one. If a general criticism can be made of the book, it is that the author is too sparing in his psychological interpretations of many of the principal characters whose eccentric qualities call for more convincing explanation than is given. Also, it must be said that the author's interest seems to flag as he approaches the twentieth century; neither the sixth nor the seventh Earl is adequately treated. Their very modest achievements might have been considered by the author in ways that he has not done. Finally, given the importance of Chevening in the life of the family, one wishes that he had been more generous in giving us additional details about the house. None of these remarks, however, ought to be considered as detracting from the usefulness of this book.

Brown University

STEPHEN R. GRAUBARD

BRITISH ORIENTALISM AND THE BENGAL RENAISSANCE: THE DYNAMICS OF INDIAN MODERNIZATION, 1773–1835. By *David Kopf*. (Berkeley and Los Angeles: University of California Press. 1969. Pp. xii, 324. \$8.50.)

ALEXANDER HAMILTON (1762–1824): A CHAPTER IN THE EARLY HISTORY OF SANSKRIT PHILOLOGY. By *Rosane Rocher*. [American Oriental Series, Volume LI.] (New Haven, Conn.: American Oriental Society. 1968. Pp. xii, 128. \$3.75.)

DR. Kopf's monograph combines the most serious analysis yet propounded of the nature of the European contribution to the nineteenth-century Bengal Renaissance with a detailed account of a most important phase in British-Indian intellectual history, a field where hitherto Eric Stokes with his authoritative *English Utilitarians and India* has been virtually a lone figure.

Kopf's thesis is that Calcutta, from the time of Warren Hastings down to the second quarter of the nineteenth century and especially after Lord Wellesley's establishment of the College of Fort William in 1800, was the scene of a vigorous and intelligent uncovering of the Indian past by such Englishmen as H. T. Colebrooke, John Gilchrist, and William Carey, and that the dedication of these scholars to the cause of Oriental learning predisposed those Bengalis who were brought into contact with them to develop a corresponding willingness to come to terms with the very different challenge presented by late eighteenth- and early nineteenth-century European civilization. The outcome was a process of cultural syncretization that constituted an important element in the early stages of the Bengal Renaissance. It is clearly very much in the author's mind that, had this process been allowed to develop, it would have been much the better for all concerned, but, in fact, this phase ended with the overwhelming assertion in official circles of that mood of evangelical and utilitarian contempt for everything Indian exemplified by Macaulay's famous Educational Minute of 1835. A

major casualty of that mood was the College of Fort William, closed down by Lord William Bentinck in 1830, and with it those traditions of Oriental scholarship that it had upheld with such distinction.

Two of the major themes of this book, the early British contribution to Indology and the rising tide of the utilitarian reaction to the company's patronage of Oriental learning associated, in particular, with Bentinck's governor-generalship (1828-1835), are not, of course, in themselves original, although Kopf has enlarged our understanding of both phases with a wealth of pertinent data and commentary. What is strikingly original, however, is his thoughtful examination of the intellectual impact of these scholarly English officials and their writings upon the educated Bengali, and herein lies the real importance of this work. It is, however, important to stress that this is a study of the intellectual elite of a single Indian region, an intellectual elite recruited from one community only and therefore exclusive of the Muslim half of the Bengali equation. It would be rash to draw from the Bengal experience general inferences about the process of modernization in the subcontinent in the early nineteenth century without recognizing that perspectives from the Maratha country, Oudh, or the Delhi Territory would be rather different.

On page 266 Kopf says that "The rediscovery of Buddhist India was the last great achievement of the British Orientalists. The later discoveries would be made by Continental Europeans or by Indians themselves." This is not a statement that can pass unchallenged since, to mention only random examples, the archaeological activities associated with the names of Cunningham and Marshall, the linguistic work of Grierson, the broad framework of the Muslim period, and much important British research in such fields as epigraphy and numismatics postdate the period of which Kopf is writing.

In Germany the pioneers in the fields of Indology and comparative philology were the Schlegel brothers, Friedrich and August Wilhelm, of whom Friedrich was for some time a pupil of Alexander Hamilton, a former officer in the company's Bengal Army who taught Sanskrit in Paris between 1803 and 1806, and then at Haileybury between 1806 and 1818. Mme. Rocher has now brought together in a slender monograph virtually everything known about this elusive figure who played so influential a role in arousing an interest in Sanskrit studies on the Continent. At a somewhat later date German Sanskritists expressed considerable contempt for the work of their English predecessors—such men as Hamilton and H. H. Wilson—but, as Rocher justly remarks, "the progress of Indian studies was a fast one, and the newcomers were soon far better armed than their predecessors, without sufficiently realizing their debt to them." In the light of these early German strictures it is, nevertheless, worth noting the substance of a recent article, "Horace Hayman Wilson and Gamesmanship in Indology," by N. P. R. Sirkin (*Asian Studies*, III [Aug. 1965], 301-23), which argues that Wilson himself, one of the foremost literary figures in early nineteenth-century Calcutta, should be regarded as an extremely plausible academic charlatan.

Yale University

GAVIN R. G. HAMBLBY

THE IMPACT OF RAILWAYS ON VICTORIAN CITIES. By *John R. Kellett*. [Studies in Social History.] (Toronto: University of Toronto Press. 1969. Pp. xxi, 466. \$11.50.)

THIS is a much-needed book. Nineteenth-century historians have long been more or less aware of the railway's central influence on the growth of the Victorian city, but now

they have at hand a lucid, detailed, and searching examination of the subject that will open up for them many fascinating avenues. Kellett has arranged his excellent book under three heads: first, the decision making involved in British urban growth, particularly by the central government, the local authorities, and the railway companies; second, case histories of the five largest British cities—London, Birmingham, Manchester, Liverpool, and Glasgow—in which the author studies the property ownership pattern, the conditions of competition between railway companies, and the development of railway facilities; and third, the railway as an agency of change in the city center, in the inner districts and suburbs, and in the urban land market. There is also a generous provision of maps as well as four useful appendixes.

Among the many good things offered the reader, it might be helpful to specify the following: the discussion in Part I of social costs, which reveals that the Victorians early learned something about the essentials of cost-benefit analysis; the careful estimates, also in Part I, of the profits made from railway building, especially by the lawyers and by landowners, large and small; the several case histories in Part II, which are particularly valuable for their analysis of the strategic significance of the property ownership pattern; the fascinating account in Part III of how the railways influenced internal flows of urban traffic, residential densities, and patterns of urban land use; and, last but not least, the first appendix, on land costs as an element in railway expenditure, which corrects some of the earlier work of H. Pollins, and the third appendix, on nineteenth-century compulsory purchase and arbitration procedures, which briefly unravels a complicated subject.

In more general terms, Kellett's book demonstrates the fruitful uses of an analytical framework that has general applicability to British urban history. It also makes clear that, to follow in Kellett's footsteps, a training in conventional political history will not be enough; some knowledge of economic theory, urban planning, geography, and the history of English law will be needed as well. In short, this book will prompt us, if we need the prompting, to reconsider how young historians should be trained.

Johns Hopkins University

DAVID SPRING

SIR JAMES GRAHAM. By J. T. Ward. (New York: St. Martin's Press. 1967. Pp. xx, 356. \$12.50.)

SIR James Graham well deserves special attention, not only because of his distinguished service in British cabinets over three decades and his closeness to Peel during the great administration of 1841–1846, but also because his immense talents and substantial accomplishments as an administrator make him of particular interest to a generation of historians increasingly concerned with the development of the Victorian administrative state. This study is not the first to be based on the Graham Papers. These papers, most of which are now microfilmed and available at several libraries in Britain and the United States, were also used by Arvel B. Erickson for his monograph published in 1952 and by Charles Stuart Parker for his two-volume biography that appeared in 1907.

Ward's book does not wholly replace Parker's since it is briefer, and, on some matters, Parker gives a fuller story. Parker was, however, uncritical and not always accurate. Ward's treatment is less pedestrian, more thoughtful, more interesting, and profits from the release of new materials and the scholarship of the intervening sixty years during which our knowledge of early Victorian politics has been greatly increased. The presentation, though a narrative rather than an analysis, sheds light on a

number of general questions and provides some interesting intellectual leads. The book is marred by certain trivial deficiencies: dates in the text are occasionally hard to follow, and expressions such as "in the autumn" or "on the 21st" have to be clarified by turning back the pages, and the author's habit of crowding sometimes as many as a dozen references into a single note often makes it difficult for the reader to ascertain which sources are cited in support of which statements or quotations. These, however, are minor criticisms of a book that will clearly be extremely useful. The account is well balanced, informed, and intelligent. It deals competently with the ambiguities, psychological as well as political, of Graham's career and offers a more complex and more perceptive portrait of him than any so far presented.

University of Iowa

WILLIAM O. AYDELOTTE

THE ROYAL NAVY AND THE SLAVERS: THE SUPPRESSION OF THE ATLANTIC SLAVE TRADE. By *W. E. F. Ward*. (New York: Pantheon Books. 1969. Pp. 248. \$6.95.)

THE Atlantic slave trade has been the subject of a number of popular treatments in recent years, in books by James Pope-Hennessy, Basil Davidson, and the collaborative effort of Daniel Mannix and Malcolm Cowley. W. E. F. Ward has written another book in the same tradition; it is focused on the Royal Navy's long campaign against the slavers. A former education officer in British West Africa, and the author of a number of books on West African themes, Ward has produced a well-written book on the subject that will nevertheless be somewhat disappointing to specialists. There is no footnote apparatus, and much of the narrative covers ground already very familiar. The book has great value, however, for the general reader. The maps and illustrations are superior, and Ward has included a glossary of nautical terms as well as sketches of sail plans that will make comprehension easier for those not familiar with the technical aspects of the sailing ship. He provides enough of the background to give his reader some familiarity with both the British antislavery movement and the state of the Royal Navy when it embarked on what proved to be one of the longest and most arduous campaigns in its history. The account of the campaign itself, illustrated by extracts from the logs of the warships involved, is balanced by brief discussions of the diplomatic and legal problems that beset the Royal Navy in its task. The author is particularly pointed about the responsibility of the United States government for allowing much of the trade to shelter under the American flag in the 1840's and 1850's. Although the naval historian will doubtless continue to rely on Christopher Lloyd's *The Navy and the Slave Trade*, Ward has provided a good account for the nonspecialist reader.

University of Delaware

RAYMOND A. CALLAHAN

ESSAYS ON GERMAN INFLUENCE UPON ENGLISH EDUCATION AND SCIENCE, 1850-1919. By *George Haines IV*. [Connecticut College Monograph Number 9.] ([New London:] Connecticut College, in association with Archon Books, Hamden, Conn. 1969. Pp. x, 188. \$7.50.)

THE colleagues of the late Professor Haines have done his reputation a substantial service in isolating from the manuscript material left at his death these six well-documented essays. Instead of denying that the English had educational "institutions" because such institutions as apprenticeship, voyages around the world, and noncredit lectures were not in standard modern form, Haines here recognizes the contrast between local, dis-

organized, voluntary institutions in England and nationwide, routinized, government-supported ones in Germany.

What emerges from the essays is the use of a standard German image by educational reformers in England in almost unchanged form from the 1860's to the middle of the First World War. Whether it be the need for scholarship in the universities, technicians for the chemical industries, the founding of new colleges, or secondary education on a mass scale, the same praise of the same characteristics of the German system seems to have come forth. What does not emerge is how effective this brand of argument was in producing any given reform.

Since it was a belief shared by many of the reformers he discusses, Haines's assumption that science meant primarily experimental or laboratory science, and in particular chemistry and its applications, is not seriously distorting in his context. We may note, however, that the new applied science during much of his period was not chemistry but electricity and that, whatever the nationality of the entrepreneurs who financed under-sea cables, they almost invariably hired English engineers to lay the cable. Not that the English dominated the electrical field, but here, as elsewhere, they were not as badly off as their reforming professors liked to say.

In short, Haines's work does not give a satisfactory general interpretation of any one event, but it challenges the student to make sure that a fuller presentation takes into account groups not usually stressed: for example, Mundella, Magnus, Roscoe, and Abel in the 1880's; the Beits, Wernher, Cassel, Mond, and Levinstein in the 1890's; and all of the individuals on government committees during the First World War.

Smithsonian Institution

WALTER F. CANNON

JOHN MORLEY: LIBERAL INTELLECTUAL IN POLITICS. By *D. A. Hamer*. (New York: Oxford University Press. 1968. Pp. xvi, 412. \$9.00.)

LORD HALDANE: SCAPEGOAT FOR LIBERALISM. By *Stephen E. Koss*. (New York: Columbia University Press. 1969. Pp. ix, 263. \$8.95.)

THOUGH these books come together for review by chance, they come most relevantly, for they are two chapters of the same story: the decline and death of Liberal England. It was a long time dying. It was already clear to John Stuart Mill, after the Reform Act of 1832, that a healthy future for the Whigs depended upon a solid alliance with the Radical party and the laboring classes, now becoming ever more powerful as the nineteenth century and the Industrial Revolution advanced. With each successive extension of the franchise, in 1867 and in 1885, the need for union became ever more obvious. Yet the former Whigs, now become the Liberal party, did nothing effective about it, even though many of the more prescient saw the need. At last, in default of Liberal support, labor formed its own party in the 1890's. Though the Liberals made a belated effort to gain the support of labor after the 1906 election with a program of welfare legislation, it was too little and too late. Death came in 1915, when Asquith established a coalition government. The Liberal party never again came to power, and the Labour party became the alternative to Conservatism in the British political system.

This is the underlying theme of both these books, but it is an unwritten one. They assume the reader's knowledge of the story, and develop, each in its own way, hitherto unexplored chapters of it. They have much else in common. Both are well written: clear, succinct, straightforward accounts, unencumbered with academic jargon. They are poised and self-assured, scholarly and erudite, newly weighing and judging great

masses of evidence, especially from contemporary newspapers and journals. But of course they are quite different books.

Morley was Secretary of State for Ireland under Gladstone, and perhaps, more than anyone else, he was responsible for the Home Rule policy that immobilized the Liberal party after 1885. He was afraid of the growing force of labor, and every time it clamored for domestic reforms he cried for justice to Ireland first. But the British public loathed Ireland and things Irish, and so the machinery of Liberalism ground to a halt and was finally overtaken by great domestic social forces it either would not or could not harness and control. Morley is therefore a very important figure in the history of Liberal decline. But this book makes no special claim for his significance, and it is not fundamentally a political analysis.

Professor Hamer has primarily written a study in the history of ideas. Morley was above all a man of letters, with a full and unshakable set of typical mid-nineteenth-century Liberal convictions, a utilitarian individualist and a Cobdenite free trader. He typically yearned, as so many sedentary thinkers do, for a life of action, of doing as well as knowing, of political power and fame as well as literary. But, when his best hopes were realized and he rose to high office, he was struck numb and could not act. Eventually he became a standing joke, even to his Liberal colleagues who nicknamed him "Priscilla" and teased him for "always hovering on the brink." Nevertheless, his career ended decisively and proudly. When war was declared in 1914, he stood firmly by his lifelong pacifism and resigned office. In this most interesting book, Hamer explores some of the ways in which thought and action mesh. He shows the failure of fixed ideas in a changing universe.

Haldane was a generation younger than Morley, coming of age in the heady era of the "new imperialism" and bent on adjusting Liberalism to it. Moreover, he was a Scot, and, what was the most fateful result for his future political career, he was trained in German universities; like Morley, a man of letters, but, unlike him, a student not of Comte and J. S. Mill, but of Hegel and Schopenhauer. Probably this was enough to keep him from the highest offices. It certainly ruined his career, for when, after a year of disastrous fumbling, the Liberal government looked for a scapegoat to excuse its failures in 1915, it easily pounced on Haldane, who had for years been caricatured in the right-wing yellow press as a bulbous, pompous, pedantic, Germanophile professor.

This episode is the central focus of Professor Koss's fluent, intelligent book. Haldane was then Lord Chancellor, but he had been Secretary of War and the architect of the British Army. His had been a heroic effort to modernize the ancient, useless militia, to create a territorial army of volunteers at a time of peace when a conscript army would have been, to the average Englishman, unthinkable totalitarian. But when Lord Kitchener was called to direct mobilization, he ignored Haldane's carefully laid plans and inevitably produced waste and chaos. It was not Kitchener, however, who suffered; it was Haldane. How and why? Koss answers these questions by thoroughly exploring the contemporary press. Otherwise so highly gifted with moral concern, intelligence, diligence, and passion for hard work, Haldane unfortunately struck many as repulsive, corpulent, and squeaky-voiced, long winded and jargonically metaphysical. Not only did he antagonize and even frighten the insular with his German interests; he was hated by the ever-growing group of conscriptionists.

Haldane was shy and modest, unable to defend himself against the vicious attacks; he lay low throughout the war and redirected his interests to national education and postwar reconstruction. But perhaps most interesting of all, he saw the impending

Liberal fate. After fruitless discussions with Asquith and Lloyd George, he concluded that neither was willing nor able to lead the Liberal party to a creative future, and so, at last, transferred his allegiance to the party of coming progress, the Labour party.

Connecticut College

MARY PETER MACK

THE COMMONWEALTH EXPERIENCE. By *Nicholas Mansergh*. [Praeger History of Civilization.] (New York: Frederick A. Praeger. 1969. Pp. xix, 471. \$12.50.)

NICHOLAS Mansergh, Smuts Professor of the History of the British Commonwealth at Cambridge, is without peer in his field, save perhaps one, Sir Keith Hancock. Each now has, within the space of two years, given students of the British Empire and Commonwealth a massive volume, massive not only in size but in conception and execution, that represents the distillation of the wisdom of a lifetime. Hancock on Smuts, and now Mansergh on the Commonwealth as a whole, will be as basic to the field as any works published in this decade. Neither volume is wholly without fault, but each represents historical scholarship at its most subtle, lucid, and appropriate. Mansergh writes as though to an unseen audience of near equals, and his book—disastrous and confusing in the hands of a beginning student, provocative and conclusive to one who can recognize the many names, references, and precedents that pass without explication—reads like a mellow, informed discussion among dons as the port is passed about, clockwise. One is expected to know that it was G. M. Young who quipped that so much diplomatic history consisted of what one clerk said to another clerk; one is expected to know who Professors Creighton, Shaw, and Ward are and what A. J. P. Taylor thought of Bismarck. This book, then, is for fellow scholars, and for them it will hold much that is new, a bit more that is familiar, and very little that could be said to be mistaken, even by nuance. It is a remarkable achievement.

The Commonwealth Experience is not a history of the British Empire. Little is said of the growth of the body that one day would become, at least in part, the loosely linked system of states called the Commonwealth. When that little is said, the focus always is on the coming Commonwealth, on that portion of the British overseas experience that fell largely to the colonies established by the white settlers, or in the period after 1911. An opening chapter sets the themes and links imperial history to that of the Commonwealth with a single question: "What was the rôle of force in the shaping of Commonwealth?" A closing chapter provides the orchestration, assessing the historical experience by asking again how a regard for liberty could have, or did, exist side by side with the ideas of Empire. Between question and answer falls a careful analysis based upon those areas of which Mansergh is the acknowledged master: of the variety of precedents by which an Empire became a Commonwealth, precedents for the most part set in, or raised by, Canada; of the unique role of Ireland (here drawing heavily upon Mansergh's own *The Irish Question*, published in 1965); of India, without which there would have been no Empire and could have been no multiracial Commonwealth; and of South Africa, that thorny state in which *The Price of Magnanimity*—as Mansergh wrote in his slim volume of that subtitle in 1962—would be constant diversion from the norm, a mirror image to the reflections of Commonwealth virtue within the Canadian story. Thus, other areas often are ignored: Sierra Leone, itself a colony of settlement, although black settlement, is mentioned only twice (and, symbolically, the index overlooks one of those instances); Burma comes in for rather too little, and New Zealand for perhaps too much, discussion. When the reader reminds himself, however, that Mansergh's subject is the

Commonwealth, its machinery, growth, and meaning, and not the more fashionable study of decolonization, the balances seem entirely appropriate.

Still, any reader might wish to have seen a bit less of some subjects and a bit more of others. Perhaps Sir Robert Menzies arrives a little too often, but then, along with W. L. Mackenzie King, Joseph Smallwood, and Sir Thomas Playford, the last two never mentioned, he is legitimately present by virtue of longevity as well as an archetype of an attitude. Not enough is made of Alexander Galt and his quite remarkable dispatch of 1859. Perhaps a discussion of the binding force exerted by the romantic fiction of Empire—by Henty, Haggard, or John Buchan (mentioned once, in the caption to one of the many excellent photographs)—would have helped show how intangibles, neither economic nor political, worked their way. Yet, if such additions had meant the exclusion of the imaginative use of Tocqueville's commentary on Canada, or of the perceptive essay on Nehru, then one might suffer one loss gladly for the riches of another reward. There is, for example, a series of exceptional photographs of the Prime Ministers' Meetings, of middle-aged or older men in ever-growing rows, at first dressed with the uniformity of the West (in 1951, only Menzies and Holland looking strange in their double-breasted suits, against the pinstriped conformity of the single-breasted others), until, in 1964, the greater strangeness of the Nehru collar itself seemed normative. There are a fine discussion of the meaning of the Suez crisis of 1956 for the Commonwealth, a timely statement about South Africa's decision to leave the club, and a poignant last paragraph that genuinely concludes and does not merely end. In the bibliography, full and critical, Mason Wade becomes F. M. Wade, and Professor Morris-Jones loses his hyphen, and there is one page (369) which quite simply cannot have been proofread. But ignore the nit picking. As Sir Charles Dilke remarked of the pakeha fly driving out the Maori fly, Mansergh's achievement rightfully makes us forget the pedant's errata lists and become, again, scholars who see how many questions there are that remain to be asked, and how well those that have been asked here have been answered.

Yale University

ROBIN W. WINKS

THE PHOENIX PARK MURDERS: CONFLICT, COMPROMISE AND TRAGEDY IN IRELAND, 1879-1882. By *Tom Corfe*. ([London:] Hodder and Stoughton. 1968. Pp. 286. 45s.)

THE Phoenix Park murders of 1882 were the most shocking assassinations perpetrated in nineteenth-century Ireland. In England they confirmed the stereotype of the Irishman as a murderous aboriginal. Mr. Corfe reconstructs, vividly and in detail, both the circumstances of the crime and the retribution that swiftly overtook the murderers of Lord Frederick Cavendish and Mr. Burke.

This very readable book is, however, more than the re-creation of a single terrible moment in Irish history. It is also the story of what Corfe terms "a revolution that failed"—a phrase by which he designates the joint effort of Gladstone and Parnell to end, through the "Kilmainham treaty," both the terrorism of the Land League and the coercion practiced by Chief Secretary Forster. As has so often happened in Irish politics, the hopes of the moderates were shattered by the violence of the extremists—in this case, by the knives of the "Invincibles."

Two-thirds of Corfe's book is devoted to unraveling the tangled web of Irish politics that resulted from the agrarian crisis of the late 1870's: Davitt's Land League, the boycott, the Fenians both in Ireland and in America, and the Home Rulers led first by Butt and then by Parnell. The dubious sources for the "Invincibles"—P. J. P. Tynan and

James Carey the informer—are lucidly analyzed. Corfe is also a fair-minded observer and does not fail to do justice to the motives of Gladstone.

He has made skillful use of English and Irish sources: the Gladstone Papers in the British Museum, the records in the State Paper Office, Dublin Castle, and in the National Library of Ireland. He has also consulted a wide range of contemporary material, including the Irish provincial press. There is a useful and up-to-date bibliography. A large number of well-chosen illustrations enhance the book's appeal. The absence of footnotes, however, makes the book more valuable for the general reader than for the specialist.

University of Washington

GIOVANNI COSTIGAN

LA PRESSE ET L'INFORMATION À TOULOUSE: DES ORIGINES À 1789.

By Marie-Thérèse Blanc-Rouquette. [Publications de la Faculté des Lettres et Sciences Humaines de Toulouse, Series A, Number 6.] (Toulouse: Association des Publications de la Faculté des Lettres et Sciences Humaines de Toulouse. 1967. Pp. x, 319.)

BOTH Arthur Young, the English traveler, and Eugène Hatin, the historian of the French press, concluded that, for all practical purposes, there was no provincial press in France before 1789. This is not so. The book of Mme. Blanc-Rouquette demonstrates that there was a "press" at Toulouse from the latter half of the fifteenth century. Until 1660 it was not a "periodical press" but an "episodic" one. Nevertheless, Toulouse possessed an array of newssheets known to contemporaries as *avis*, *nouvelles*, *canards*, or *feuilles*, seldom exceeding the four-page pamphlet, often illustrated, selling for a few sous, and reaching an audience even beyond the city walls. These *occasionnels* continued to circulate even after Renaudot's *Gazette* (1673-1752) and the *Mercure galant* (1694-1710) of Paris were published at Toulouse.

The importance of religious issues at Toulouse was not confined to the Wars of Religion, but was demonstrated by a continued interest in Jansenism, Gallicanism, the Jesuits, and in the famous Calas affair. Changes in public taste are suggested, however, by a gradual shift from treaties and battles to more serious attention to events in foreign lands, especially in Stuart England. By the reign of Louis XIV, announcements of army promotions, court presentations, and royal visits were complemented by allusions to solar eclipses, earthquakes, and comets, indicating a new interest in popular science. Brochures were often inspired by the parlement, adding a polemical dimension to the more mundane extracts of law cases, judgments, or ordinances of the Crown. Local literary quarrels also appear around 1700.

To me, the most interesting part of this study is the author's investigation of the *Affiches de Toulouse*, a weekly published from 1759 until the Revolution. The *Affiches* consisted primarily of what we call today "classified ads" publicizing sales and rentals of seigneuries, offices, annuities, auctions of all kinds, prices of foodstuffs, solicitation of textileworkers, promotion of commercial ventures, and the like. The *Affiches* also catered to public interest in medical discoveries, scientific exploits, book and theater reviews, fashions in dress, in addition to anecdotes, curiosities, and *petites histoires*. Here is an excellent, and largely unused source of social history.

Altogether, this is more of a chronological catalogue of news content than it is a topical analysis of trends in public opinion, changing mores, new vocabularies, or a systematic effort to measure the diffusion of ideas. But, supported by extensive quotation and a score of telling reproductions, it is a very useful catalogue that opens a new field for exploration.

Johns Hopkins University

ROBERT FORSTER

LA GRANDE CHANCELLERIE ET LES ÉCRITURES ROYALES AU SEIZIÈME SIÈCLE (1515-1589). By *Hélène Michaud*. Preface by *Georges Tessier*. [Mémoires et documents publiés par la Société de l'École des Chartes, Number 17.] (Paris: Presses Universitaires de France. 1967. Pp. viii, 419. 40 fr.)

HÉLÈNE Michaud has undertaken an extremely difficult project, namely, to examine the role of a key institution in the process by which early modern French rulers formulated orders for their subjects. I am torn between praise for the author's ability to uncover all of the facts which the mass of disparate sources contain and nagging doubts concerning her institutional-legal approach to their interpretation.

The conceptual weaknesses are most evident in the first half of the study. We learn something about the multitude of chancery officials, their functions, social setting, and literary interests. But one wonders whether a balance between legal forms and political realities might have yielded more insight into the subject than the rigid catalogue of "types" of officials provides. Certainly it seems unrealistic to discuss the chancellors and keepers of the seals in separate categories, when in fact the *garde des sceaux* was a substitute for the *chancelier* and frequently was elevated to the chancellor's office in due time. Ironically, the best chapter of this section is devoted to the secretaries of state, whose role as composers of unsealed letters was steadily eroding the importance of the *Chancellerie*, entrusted with the Great Seal. In tracing the evolution of those work horses of the *ancien régime's* administration, Michaud supersedes all previous institutional studies on the subject.

The second half of the book is more revealing. The author's emphasis on legalities is moderated by her acute awareness that the formulation of royal *actes* was a complex process. With painstaking care, Michaud shows the relative roles of the chancellor and his assistants, the secretaries of state and their staffs, the royal councils, and the king or the queen mother. With a judicious mixture of detail and summaries, the author describes the entire mechanism, from initial decisions through drafts and memorandums to signing or sealing, registration and compilation of governmental archives. One cannot criticize the author for the picture of administrative confusion and frequently uncertain inspiration that re-creates the administrative-legislative life of the late Valois monarchy. It is to be hoped that political historians will use this book as a guide for fresh studies on the sixteenth-century men who had to contend with such a complicated governmental process.

University of Southern California

A. LLOYD MOOTE

À TRAVERS LA NORMANDIE DES XVII^e ET XVIII^e SIÈCLES. By *Michel Bouvet* and *Pierre Marie Bourdin*. Introduction by *Pierre Chaunu*. [Cahier des Annales de Normandie, Number 6.] (Caen: Centre de Recherches d'Histoire Quantitative de la Faculté des Lettres et Sciences Humaines de l'Université de Caen. 1968. Pp. 522.)

CLEARLY a work for the specialist, this is the sixth volume in a series primarily concerned with the demography of Normandy in the seventeenth and eighteenth centuries. An introduction by Professor Pierre Chaunu, director and general editor of the series, very briefly explains the nature of the problems involved and the methods used to attack them and summarizes the work completed in the volumes already published.

The heart of this volume is two closely written monographs. Michel Bouvet's

Troarn: Étude de démographie historique (xvii^e-xviii^e siècles) concerns a group of rural parishes along the lower reaches of the Dives River about seven miles east of the city of Caen and a similar distance from the mouth of the Dives. Frequent and prolonged flooding of the Dives often brought crop failure; famine threatened or occurred; and endemic diseases such as paratyphoid approached or reached epidemic proportions. Bouvet has skillfully and ingeniously combined the evidence extracted from tax rolls and parish registers to provide a previously unequalled portrayal of conditions in these beleaguered parishes and to suggest methods by which such evidence can be more effectively used to provide greater accuracy in determining a host of basic vital statistics such as birth and death rates, sizes of families, and ages at death. The second monograph, Pierre Marie Bourdin's *La plaine d'Alençon et ses bordures forestières*, employs many of the methods and techniques used by Bouvet. The area studied, however, enjoyed a more prosperous agriculture than Troarn and derived income from mines, forges, metalworking, and textiles. In addition to providing vital statistics on these parishes, Bourdin has supplied a masterfully detailed and intensive study of the effects of typhoid epidemics on these families and the economy and the surprisingly energetic efforts of the royal government to end the "fièvres putrides." Included are verbatim reports of physicians, clergy, and government officials detailing symptoms and treatment of the afflicted and an excellent glossary of seventeenth-century medical terminology.

The authors apparently have made good use of existing sources, and both monographs bristle with tables and graphs in addition to all the usual documentary apparatus. The poor quality of several map reproductions and the confusion of town names with those of parishes and topographical features may initially perplex the reader, but he will find that Bouvet and Bourdin draw only carefully supported conclusions, which appear to give little comfort to the partisans of either Porshnev or Mousnier.

Pennsylvania State University

ROBERT W. GREEN

ASPECTS DE LA CONTREBANDE AU XVIII^e SIÈCLE. By Marie-Hélène Bourquin and Emmanuel Hepp. Prefaces by François Dumont and Jean Imbert. [Tra-vaux et recherches de la Faculté de Droit et des Sciences économiques de Paris. Series "Sciences historiques," Number 14.] (Paris: Presses Universitaires de France. 1969. Pp. viii, 96. 10 fr.)

THIS volume contains two studies that deal with the phenomenon of smuggling in eighteenth-century France. *Le procès de Mandrin et la contrebande au xviii^e siècle* by Marie-Hélène Bourquin focuses on Louis Mandrin, chief of a contraband operation in Dauphiny who was broken alive on the wheel in 1755. As the author demonstrates, Mandrin was neither the first nor the last to be so savagely punished. The documents show that between 1733 and 1771 fifty-seven smugglers were thus horribly executed. Mandrin's activities were no more exceptional than the mode of his punishment. Throughout the century smugglers, sometimes in large troops, crossed into France, posing a threat to revenue and to public peace. Glamorized by legend, they were in fact often brigands. But Mandrin had a good press. He was captured in Savoy, and the ensuing diplomatic imbroglio publicized his name. Further, Mandrin was tried by a special tribunal that violated normal jurisdictions; the magistrates made Mandrin a hero in their political polemics. Bourquin, using well-known materials and new documents, tenders a careful, modest study of the most famous of French smugglers.

Emmanuel Hepp's *La contrebande du tabac au XVIII^e siècle* studies tobacco smuggling as a function of the establishment of the tobacco monopoly in 1730 and of the astonishing spread of the tobacco habit. Revenue from the monopoly quadrupled from 1730 to 1789, an increase paralleled by illegal sales financed, Hepp suggests, by many highly placed personages whose shadows at least he reveals. Hepp describes the General Farms' antimuggling machinery from policy making in Paris to the work of agents in seaports and inland taverns and discusses the operation of the penal code developed to curb, by judicial terror, the crime of smuggling. Yet smuggling was at best merely contained within tolerable limits. Hepp mines details from hitherto unexploited sources, but, except in pointing to the international organization of the illicit trade, he provides little that is especially new.

Oakland University

GEORGE T. MATTHEWS

FROM THE ANCIEN RÉGIME TO THE POPULAR FRONT: ESSAYS IN THE HISTORY OF MODERN FRANCE IN HONOR OF SHEPARD B. CLOUGH.

Edited by Charles K. Warner. (New York: Columbia University Press. 1969. Pp. vii, 211. \$7.50.)

SHEPARD B. Clough, a member of the department of history at Columbia University since the 1920's, has probably trained more historians of modern France than any other American. Ten well-known scholars among them now honor him with a *Festschrift*, and in it they have demonstrated that they are worthy followers of a gifted master. Each essay is soundly based and well written, each makes a contribution, and the entire *Festschrift* reveals the hand of a skilled editor.

The first essay, "The *Encyclopédie* as a Business Venture" by Ralph H. Bowen, draws on manuscript sources and some published documents to tell the fascinating story of what was probably the largest publishing venture undertaken to that time and one of the great enterprises of the eighteenth century. In "The Meaning of the Revolution: Seven Testimonies" Paul H. Beik summarizes the judgments of seven contemporaries on the Revolution: Bonald, Mounier, Barnave, Sieyès, Condorcet, Robespierre, and Babeuf. He finds among them considerable agreement on the basic meaning of the Revolution but disagreement on what could and should replace the corporative society crumbling about them. Dora B. Weiner in a revealing article entitled "French Doctors Face the War, 1792-1815," explores the training, organization, and role of French doctors in the Revolutionary and Napoleonic Wars, government policies toward medical men, and changing popular attitudes toward them. In an essay on "The Growth of the French Securities Market, 1815-1870," Charles E. Freedman shows that, contrary to long-accepted opinion, Frenchmen in the decades after 1815 were not investing almost exclusively in land and the family mattress. The market for government bonds reached national proportions by mid-century, and, beginning in 1830, there was much investment, some of it surprisingly bold, in private companies. The editor, Charles K. Warner, has contributed an article on "The *Journal d'Agriculture Pratique* and the Peasant Question during the July Monarchy and the Second Republic." He adeptly combines his exposition of the journal's activities with consideration of the government's agrarian policies, making the essay a contribution to a little-explored area of agricultural history. In "The Alsace-Lorraine Question in France, 1871-1914," Frederic H. Seager emphasizes that French diplomacy minimized the question of Alsace-Lorraine, keeping it ever subordinate to the fundamental French aim, after 1871, of finding allies. The general public, moreover, was largely

indifferent to the lost provinces; even the efforts of irredentists during the period of the nationalist revival before 1914 failed to stir much interest.

To me, the most interesting essay in the collection is Raymond F. Betts's "The French Colonial Frontier." He shows that in the eyes of colonialists the French colonies were to play in French life a rejuvenating role similar to the reputed influence of the American frontier on American life. French society of the late nineteenth and early twentieth centuries was, they held, sickly, anemic, and decadent; colonial life was healthy, manly, and energetic. In the colonies, especially in Africa, men of action could find fulfillment and satisfaction, and they would then, the colonial enthusiasts believed, contribute to the revival of vigor and purpose in metropolitan France. Sabine Jessner presents a chapter of the life of one of the great men of the Third Republic in an appealingly lucid article on "Édouard Herriot in Lyons." Although as a leading spokesman for Radical Socialism on the national scene Herriot always denigrated state intervention, as mayor of Lyons he vigorously practiced intervention. Exploiting municipal power to the fullest, he undertook to develop the economy of the city and to improve the condition of the poor, personifying in this part of his career the authoritarian tradition of Jacobinism. Martin Wolfe's essay, "French Interwar Stagnation Revisited," contributes to the ever-lively debate over French economic backwardness. He considers whether the stagnation of the 1930's—the 1920's were a period of remarkable economic growth—was owing to deficiencies of French character and values or to events and governmental policies, and he concludes that both contributed and that present methodology does not permit determining which was the more important. In "Politics and Economics in the 1930's: The Balance Sheet of the 'Blum New Deal'" Joel Colton reviews the judgments of current scholarship on the Popular Front and points up questions still unresolved. In concluding he subscribes to Jean Bouvier's judgment, "short-term failure, long-range success"; the inspiration of the Front's defense of democratic liberties and its struggle for social and economic democracy survived the failure of 1937.

At the end of the volume the three-page bibliography of Clough's publications is testimony to his productivity and to the variety of his interests. It is an impressive record for any man, and there is no reason to believe that it is finished.

University of Washington

DAVID H. PINKNEY

LE REMPLACEMENT MILITAIRE EN FRANCE: QUELQUES ASPECTS POLITIQUES, ÉCONOMIQUES ET SOCIAUX DU RECRUTEMENT AU XIX^e SIÈCLE. By *Bernard Schnapper*. [Bibliothèque générale de l'École Pratique des Hautes Études—VI^e Section.] (Paris: S.E.V.P.E.N. 1968. Pp. 326. 45 fr.)

At a time when the US is switching to a military lottery, this judicious monograph on the operation of the nineteenth-century French recruitment system based on a draft by lottery provides an interesting yardstick. Most historians are peripherally aware that paid military substitutes did exist, but few of us have clear notions as to how such a system was justified, how it could be reconciled with military requirements, what social groups it affected, and how it was run. It is the merit of Bernard Schnapper's book that he raises and answers these and many other such questions.

The more conventional parts of the monograph (Parts I and III) deal chronologically with the problem of military recruitment as it developed through legislation, administrative practice, and public debates down to the final abolition of paid substitutes in 1872. It provides some interesting insights into most of the major military

controversies of the period in which ideology (mostly Jacobin), social convenience (to the possessing classes), military efficiency (variously defined), and cost (cheapness first) were the chief variables. In this respect, Schnapper's work leaves off about where R. D. Challener's *The French Theory of the Nation in Arms (1866-1939)* begins.

The most unfamiliar section of the monograph (Part II) focuses on the system of military substitutes itself: its scope (about a quarter of the yearly contingents were hired replacements); its geography (prosperous substitute-importing regions like Normandy drawing on poor but militaristic areas such as Alsace); its sociology (the replacement often drawn from the declining artisanate, the replaced from the middle classes of town and country); its economics (with detailed consideration of price ranges and fluctuations); its entrepreneurial history (involving elaborate companies that not only supplied substitutes but also ran mutual insurance schemes to insure against the risk of having to furnish a substitute). The author also compares the French system with recruiting practices elsewhere in Europe—a welcome departure from traditional French ethnocentrism.

Schnapper views the system of military substitutes as the outcome of a double compromise: a compromise between the revolutionary heritage of the nation-in-arms and the interests of the well to do; a compromise between the army as the cross section of the nation and one of long-term mercenaries. The Jacobin slogans may in fact have cloaked the taxpayers' reluctance to shell out for reasonable soldiers' pay. Certainly the well to do wrapped themselves in liberty to defend the right to buy their way out. Yet so long as the yearly lottery exempted substantial numbers of young men from the draft and the risks of military life remained low, the system of substitutes flourished. Antiegalitarian as the system was, in contrast to current college deferments, at least the rich sweetened the pill by *hiring* the poor to serve in their stead.

Lucid, well organized, and richly documented as this monograph is, in places it becomes quite technical, and, to me, overly detailed. Yet even general historians, who may not really want to know as much about French military recruitment as Schnapper insists on telling, will be grateful to find the book on the library shelf, if and when. Students of nineteenth-century military administration will find it more immediately absorbing, while business historians may want to look into it since, at least for France, the system of hired substitutes may have been the main stimulus to the development of life insurance companies.

University of Michigan, Dearborn

PETER H. AMANN

PÉTAINE THE SOLDIER. By *Stephen Ryan*. (South Brunswick, N. J.: A. S. Barnes and Company. 1969. Pp. 315. \$8.00.)

THE career of Pétain the soldier can conveniently be divided into two parts: one leading up to the victory of 1918, the other encompassing the period from the armistice to the aged marshal's death in 1951. Ryan adopts this method of treating his subject and presents Pétain sympathetically as an officer whose keen military mind led him to recognize the importance of matériel and the defensive in modern war long before 1914 and whose moral courage in refusing to compromise his convictions by embracing the then fashionable *offensive à outrance* theories led him to the brink of retirement from the army. Next, Ryan traces Pétain's development, during the great conflict, into a superb technician in the art of war and into one of the great strategists of his time. Here, Ryan performs a real service in dispelling once and for all the myth of Pétain the "defeatist" during the dark days of 1917-1918; as commander of the French

Army, he brought it back from demoralization, despair, and mutiny and refused to force it into fruitless offensives that would have permitted the Germans to win the war in the spring or summer of 1918.

After the muted victory of that year, Pétain became the most important military man in France, remained in this position even after he retired from active duty in 1931, and, following the defeat of 1940, became the country's master in the authoritarian regime based at Vichy. It is in this second part of Ryan's analysis that, despite some excellent and well-balanced sections on fortifications, manpower, and command organization, serious flaws appear. One flaw is an inadequate scholarly apparatus: scanty footnotes, no bibliography. It is difficult in many sections to see what sources the author relied upon for his conclusions. It is apparent that he consulted no original documents in the French archives and, rather than using such recent analyses based on these papers as those done by Pedroncini (on the mutinies of 1917), Tournoux (on the eastern fortifications), and me (on the effectives and disarmament questions), he relied on older materials. Then, too, the lack of documentation often leaves the reader in complete ignorance of the fact that there are other quite valid points of view than those advanced by the author. This is especially true concerning the question of responsibility for the defeat of 1940. Ryan presents the conventional French soldiers' "psychopolitical" explanation, which minimizes the crucially important factor of doctrinal ideas (ideas for which Pétain was largely responsible).

Another very basic flaw is that Ryan never seems to have thought through the complex problem of the soldier in the twentieth century, where war has expanded to include political, social, and economic factors. The "good soldier," in Wavell's meaning of the term, is one who elaborates a technically accurate doctrine despite unfavorable nonmilitary factors and who plans "pure or military" strategy. Another kind of soldier, however, is one who neglects his professional duties in the pursuit of economic, political, social, and "grand" strategical questions that are properly the concerns of civilians. It is possible to see an outstanding example of this second type of soldier in Pétain, who slipped the moorings of the "good soldier" after 1918 and became a public figure. In the process of this evolution he lost his grip on his craft, prepared the French Army for another war of 1918, and failed to speak out in favor of a valid although politically unpopular doctrine and military organization. He irretrievably weakened the regime by his criticism and dabbling in military sedition and, in a fine show of indignation as the "indispensable man," wrested control of France from the demoralized statesmen of the Third Republic in 1940 without ever having had to "plot" a seizure of power. His regime did not prevent the Polonization of France by the Nazis. His trial in 1945 did not restore French pride, as the author claims; that is what Gaullism is all about.

It is this Pétain whom Ryan's book does not recognize. His analysis, then, contains excellent work on Pétain the soldier up to 1918, but, thereafter, it often reads like a pro-Pétain tract in the manner of General Alfred Conquet or Colonel André Billard. Pétain still awaits his biographer; this book does not fill that enormous gap in modern French history.

Trinity College, Hartford, Connecticut

PHILIP C. F. BANKWITZ

LES COMMUNISTES FRANÇAIS: ESSAI D'ETHNOGRAPHIE POLITIQUE.
By *Annie Kriegel*. ([Paris:] Éditions du Seuil. 1968. Pp. 319.)

A VENERABLE fixture on the French political scene, the Communist party still arouses

mixed and passionate emotions: it inspires fear among parties to its Right, especially among certain socialists who cannot shake their helpless addiction to the conviction that the Left can be a major force without the Communists; its leaders have been dismissed as "Stalinist creeps" and the party described as a lumbering bureaucratic machine geared to the preservation of its parochial interests by student revolutionaries; and ideological cleavages within the party have not been uncommon. Still, little is known about the party and its history—the fine works by Wohl, Brower, Cauter, and Madame Kriegel herself are concerned with specific periods, and the general histories by Fauvet and Walter are less than satisfactory—and there are no studies of the party leadership, its decision-making process, recruitment, clientele, and functionaries over its history of almost fifty years. The author, herself a former Communist *militante* and author of well-received studies on the origins of French Communism, the development of the CGT in 1918–1921, and the response of workers to World War I, attempts in this volume to explore the structure and nature of the party since its birth at Tours in 1920.

This is not a "conventional" political history; nor is it a chronological recitation of trends and developments, although it bears chronological markings and emphasis is given to the period between the two world wars. A general work, suggestive in tone and incomplete by definition, it is an analysis of a society with its own severe codes of conduct, hierarchy, modes of recruitment, educational system, a "party-society" that has come to prize homogeneous mediocrity as the stuff of which revolutionaries are made and is eager to preserve its members from corrupting outside influences and from its own history. Kriegel examines the constants in the party's electoral geography, the diverse social origins of its members, and the "dynamic," almost spiritual character of membership, the synthetic tensions created to preserve a sense of participation in a great collective adventure of deliverance, and the stages in the party's as yet incomplete emancipation from Soviet tutelage. Especially interesting is her delineation of generational cleavages within the party, so sharp that it is the home of well-defined groups, each having had a different historical experience. The "generation of 1924–1934," the "Bolshevizers" personified by Thorez (although Kriegel is very ambiguous in her assessment of the departed chief) and Waldeck Rochet, still shapes the party's destinies and betrays all the infirmities of age, including the desire for law and order, especially their own. Thus, the party is resolutely "conservative," lacks a "revolutionary model," is overtly hostile to the aspirations of the young, and serves basically as a lobby in the interests of labor; hence, the non-revolutionary behavior of the party in the "events of May" 1968. Included are accounts of works in progress and completed, research materials, and statistical information. In substance, this valuable beginning to the writing of the history of the PCF suggests the kind of work that political historians will increasingly be expected to do.

Wesleyan University

NATHANAEL GREENE

THE POLITICS OF EDUCATIONAL REFORM IN FRANCE, 1918–1940. By John E. Talbott. (Princeton, N. J.: Princeton University Press. 1969. Pp. x, 283. \$9.00.)

THIS is an interesting, well-organized, and objective study of the origins, purposes, and consequences of the educational reform movement in France between the two world wars and of the reactions of political parties and other groups to reform proposals, especially to the idea of the *école unique*. The need for such reform is shown in

an introductory chapter in which the author gives an admirably clear and concise description of the French educational system in the nineteenth century. The lower classes were excluded for the most part from secondary and higher education by fees, lack of scholarships, and differences in curriculum between the public elementary schools and private elementary classes of the *lycées*. No concerted drive to reform this system, through which the bourgeoisie maintained its social and political predominance, was made until the shock of the First World War prompted some young army officers who had been teachers to found an association of *Compagnons de la Nouvelle Université* to work for a common primary school, free secondary education, and the selection of students for secondary education on the basis of merit. Some of these proposals were endorsed by the Radical Socialists and Socialists, by the CGT and numerous educational associations, and, in the late twenties, by some Catholic intellectuals and Popular Democrats. The author draws upon many newspapers, periodicals, memoirs, and parliamentary debates, as well as interviews with participants in the reform movement, for his analysis of their ideas and activities.

Although tuition was gradually abolished in *lycée* classes by 1933 and experimental orientation classes were established in 1937, Professor Talbott concludes that the movement failed largely because of the weakness of the alliance between Radicals and Socialists and their anticlericalism; Catholic fear of a state monopoly of education and the demand for subsidies for confessional schools; the desire of the bourgeoisie to maintain its power through a social, educational elite; financial and social problems; the coming of the Second World War; and the failure to study seriously curricular reform, the relationship between social classes and educational achievement, and the effects of social mobility. But the experience of the interwar years prepared the way for the reforms of the Fifth Republic.

Two general histories of French education since the French Revolution appeared in 1966 and 1968, one by Félix Ponteil and the other by Antoine Prost. An article by D. R. Watson of the University of St. Andrews entitled "Educational Reform in France, 1900-1940," was published in *Past and Present* (XXXIV [July 1966], 81-99), followed by an interesting debate between Talbott and Watson (*Past and Present*, XXXVI [Apr. 1967], 126-37). Watson sketched in outline the political history of the educational reform movement, but differed from Talbott in his appraisal of the roles of the Radicals and Socialists, in his omission of the CGT, and in some of the reasons he gave for the failure of the reform movement, which reflect British experience in education. Talbott's book now permits a more thorough discussion of some of the issues raised in this exchange. The opening of the official archives would be required for a completely definitive work.

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EVELYN ACOMB WALKER

PIERRE LAVAL AND THE ECLIPSE OF FRANCE. By *Geoffrey Warner*. (New York: Macmillan Company. 1969. Pp. xvii, 461. \$8.95.)

CRISIS AND DECLINE: THE FRENCH SOCIALIST PARTY IN THE POPULAR FRONT ERA. By *Nathanael Greene*. (Ithaca, N. Y.: Cornell University Press. 1969. Pp. xv, 361. \$11.00.)

GEOFFREY Warner, who teaches politics at the University of Reading and who has published a number of interesting articles in *History Today*, correctly stresses in his preface the urgent need for an objective, dispassionate, and scholarly evaluation of the career of Pierre Laval. The harsh criticisms and opprobrium cast upon Laval

during the Second World War and the near-lynch atmosphere of his unfortunate postwar purge trial have left his name a virtual synonym for treachery and opportunism. With remarkable diligence, Warner has scrutinized all the available evidence—the memoirs, the diaries, the testimony at the liberation trials, the depositions before the parliamentary investigations, and the available documents from the Foreign Office, and he has questioned as well a number of surviving participants.

He uses his sources critically and applies extreme caution in his deductions; some of his research corrects secondary authorities like Robert Aron or trips up the memory of an Anthony Eden. And the anecdotes are all there, or at least most of them are that give flavor to the story. Yet, despite the energy with which this much-needed enterprise has been pursued and which indeed traces in rich, valuable, and well-documented detail every facet of Laval's career from the modest dwelling in Châteldon to the execution courtyard at Fresnes, the net result is not completely successful principally because the author fails to keep his protagonist in focus. The result, an extended, detailed account of the course of French affairs, principally diplomatic, in the 1930's and 1940's, does not sufficiently illuminate the character and motivation of one of the most challenging and baffling political figures in contemporary history. Even the conclusions that emerge are not as sharp or convincing as they might be. One, the forced analogy between Laval's vision of a unified Europe and De Gaulle's, is not worthy of the author. The reason for Laval's execution at the time of liberation is curiously explained away "on grounds of *Realpolitik*." With one of his overarching explanations for Laval's political behavior there can be less quarrel. Warner argues that Laval was burdened with "an unlimited faith in his own infallibility." This supreme self-confidence had been growing on him since 1935, and he stubbornly persisted in his illusions until they all collapsed around him. One of the saddest of these illusions, as most of us had long suspected, was that he "failed to see that there was no basis for rapprochement with Hitler's Germany in terms other than the humiliation of France." At the same time, Warner, arguing that it is "more than unfair" that Laval has gone down in history with the reputation of a devious intriguer, mystifies us with the cryptic remark that the greatest flaw in Laval's character was "not deviousness, but a frightening tendency towards over-simplification." Never does he clarify for us the nagging question whether Laval's behavior is to be attributed to "over-simplification" or to disingenuousness, nor remove the suspicion that Laval epitomized a Machiavellian duplicity in which no one, not even Laval himself, knew which prince he was really serving. But despite these criticisms everyone will be indebted to Warner for his painstaking examination of the record.

The second book, written by Nathanael Greene, a very promising scholar who teaches at Wesleyan University, does not suffer from lack of focus on its main theme. Ironically, it was in 1936, at the moment of the Socialist party's greatest electoral triumph, that it began to undergo a crisis of dissension over international affairs that reached a climax in the Sudeten affair and finally rendered the party virtually impotent in the critical two years from September 1938 to July 1940. The internal disagreement sharpened into a cruel struggle between the two principal leaders of the party, Léon Blum, the prestigious head of the party, and Paul Faure, the party's energetic, able, and devoted general secretary. Together the two had rebuilt the shattered SFIO after the Communist secession of 1920; had formed a viable center protecting the party from outright reformism on the one hand and the revolutionary extremism of Jean Zyromski's *Bataille Socialiste* and Marceau Pivert's *Gauche Révolutionnaire* on the other; and had propagated the pre-1914 socialist message of

peace, antimilitarism, and internationalism. When the harsh realities of fascist aggression raised new international challenges in the 1930's, Faure persistently clung to the older socialist pacifism and antimilitarism. Blum reacted differently but, as every responsible writer would concede, not all at once. (Perhaps not everyone could have the supreme self-confidence of a Laval!) The change was slow and tortuous, the Spanish crisis made the process even more complicated, and the exact moment of the transformation, as the author correctly notes, is difficult to determine. What is clear is that after October 1938 the *Blumiste* and *Fauriste* factions were in bitter conflict although the myth of party unity prevented open schism. Nothing, not even the declaration of war itself, could bring Faure and his followers to renounce their pacifism and defeatism.

With care, precision, and insight Greene has examined the painful record of the party during these years, using as his principal sources not only *Le Populaire* but the provincial press in practically every departmental federation of the party as well. He has scrutinized the party debates in its national congresses, its national council meetings, and at the federation level. He has tabulated election returns, fruitfully explored aspects of electoral geography and electoral sociology, and investigated the economic background of those federations that took the harder line in foreign policy. Most of these efforts are successful although the examination of attitudes on the Spanish nonintervention policy are too complex to yield readily to statistical or geographical analysis. There are occasional lapses, such as some minor errors in the career of Blum. For some reason the author fails to mention the crushing of the general strike of November 30, 1938, which drove an even deeper cleavage between the Daladier government and labor, and made Blum's task of rallying support for Daladier's post-Munich foreign policy that much more difficult. Perhaps some of the judgments are a little extreme as if the author cannot empathize adequately with the agonizing ideological and practical dilemmas involved. Perhaps, too, he might have emphasized some of the deeper tensions and insoluble dilemmas of twentieth-century socialism, of international affairs in the 1930's, and of the French nation itself in that era. The crisis and decline of the SFIO, which he so ably analyzes, coincided with the eclipse of France, to use the subtitle of Warner's book; the victim was not only the French Socialist party but France itself; and in the end, at least for a time, the victor was neither Blum nor Faure but Laval. Both these books are welcome and important contributions to the tragic history of France in the late 1930's.

Duke University

JOEL COLTON

STATEN OG KUNSTNERNE: BEVILLINGER OG MENINGER UNDER ENEVÆLDE OG FOLKESTYRE. By *Aage Rasch*. [Skrifter udgivet af Jysk Selskab for Historie, Number 22.] (Aarhus: Universitetsforlaget. 1968. Pp. 218. 40.50 D. kr.)

THIS is primarily administrative history. The part played by Danish kings who, during the Renaissance and the era of despotism, supported artists and performers fills a first chapter of thirty-one pages. Individuals received grants for study abroad, for the completion of works in music, sculpture, and painting, and for their daily needs. In time of peace, about 4 per cent of the budget went to these subsidies; in wartime usually about 2 per cent did.

In four chapters totaling 134 pages, the support given by democratic governments from 1848 to 1964 is summarized. At times these funds seem to have been employed

as "old-age pensions" for artists who had nothing else to live on; on occasion they were given as rewards for outstanding achievements, most often to authors; very seldom did they strengthen some certain nuance of expression favorably viewed by the ruling powers. In this period only four of the ministers who controlled these resources stand out as persons who knew and valued artistic work and who gave time and thought to use of the funds so that the nation would profit. During this time it was expected that, when the budget ran short of money, the proviso for aid to artists was one of the first and the easiest to reduce or eliminate.

The study ends with chapters on travel stipends allotted and on economic observations called forth. A most important appendix gives the sums that, each year from 1850 to 1965, were spent for these various aids to artistry and culture. The literary style is, in general, rather pedestrian, but it rises well above that when the author deals with some of the lively discussions that new directions in art brought about. The work is enriched by twenty-seven pictures on twenty pages; it has a pleasing type style, and it is attractively bound. The proofreading and the indexing have also been carefully done.

Rasch disclaims any attempt to evaluate the work that these subsidies and stipends brought into being, or how national life was affected by those who traveled. Nor does he move widely around his theme. In general, the influence that the religious fundamentalism or Grundtvig and Schartau had in a puritanical, antiartistic direction receives little attention, and there is no clear treatment of the changes brought about by the rise of Marxism, Socialism, and Communism. Only in relation to subsidies given Johannes Jensen and Martin Andersen Nexø does this emerge.

The conclusion that was not spelled out by the author but is easily drawn from text and cash accounts is that art fares ill under elective governments. Beginning with 9 to 10 per cent of the budget during the first years of democracy (1848-1852), the sums decreased from a little over 5 per cent in 1875 to less than 3 per cent in 1900; it continued to decrease from below 1 per cent in 1912 to one-eighth of 1 per cent in 1950, and it has been even less than that since. In Denmark, culture seems to be one of the things that the proletariat neither buys nor supports. Dozens of studies such as this need to be made, published, read, compared, and thought about. Perhaps their lessons might be acted upon.

University of Southern California

FRANCIS J. BOWMAN

POPULATION AND SOCIETY IN NORWAY 1735-1865. By *Michael Drake*. [Cambridge Studies in Economic History.] (New York: Cambridge University Press. 1969. Pp. xx, 255. \$11.50.)

THE assumption that Western European societies experienced high birth and death rates until late in the eighteenth century and that the accelerated rate of population growth thereafter resulted from a sharp decline in the death rate made possible by advances in medical science has recently been challenged in some quarters. Population trends in eighteenth- and early nineteenth-century England and Ireland have led scholars to suggest that the preindustrial West may have limited fertility in an effort to increase per capita income. Before this view or others can be verified, it is necessary to subject all available statistical material in the West, together with the often faulty observations of contemporaries, to a critical examination. Dr. Drake's excellent study is therefore part of an "international discussion" that has come to focus on the quality of the sources used by demographic historians.

Drake chose Norway as his special field of investigation because that country has a remarkably complete collection of printed and unprinted statistics and supplementary population material for its preindustrial period—from 1735 to 1865. He was aided greatly by the careful studies of Eilert Sundt, “a pioneer of empirical sociology,” in the third quarter of the nineteenth century. The author finds the census returns and the compilations of vital statistics in Norway to be in substantial agreement and reliable, despite minor shortcomings.

The Norwegian population, which had increased at a rate varying from 0.4 to 0.7 per cent per year from 1735 to 1815, doubled in the period from 1815 to 1865. The immediate cause of this explosion was a sudden, sharp, and final fall in the death rate. Until 1865 the country was able to withstand the growth in population because of the shift of labor from agriculture into the traditional fields of shipping, lumbering, and fishing and the expanding potato production. Even more significant, Norway doubled its cultivated area during the period 1820–1865 and introduced new equipment and methods of cultivation, thus averting a Malthusian lowering of living standards. After 1865, however, the national task became much too difficult, and in the late nineteenth century Norway was losing, through emigration, a larger part of its natural human increase than any European country except Ireland. Fertility appears not to have varied much in the years 1735–1865. Levels of nuptiality and the age of marriage remained relatively stable, and neither the birth nor the death rate was anywhere near as high as in most underdeveloped countries today.

Marriage in Norway seems to have occurred in the late twenties for men and in the mid-twenties for women. Farmers tended to marry later in life than cotters but to choose younger women. The author found the households of both farmers and cotters to be smaller than was believed by Malthus and other observers. Similarly, children did not leave home to take employment as early as has been traditionally believed.

The author concludes that Norwegian population developments over more than a century apparently had little impact during the early years of industrialization. Preindustrial patterns of age at marriage carried over into the new era. Fertility remained relatively unchanged or moved to somewhat lower levels, and the death rate was affected only slightly until the present century.

The book contains useful maps, ample summary tables in the text, more complete statistical material in the appendix, and a bibliography.

St. Olaf College

KENNETH O. BJORK

DAS TEXTILGEWERBE IN HESSEN-KASSEL VOM 16. BIS 19. JAHRHUNDERT. By *Ottfried Dascher*. [Veröffentlichungen der Historischen Kommission für Hessen und Waldeck, Number 28. Quellen und Darstellungen zur hessischen Sozial- und Wirtschaftsgeschichte, Number 1.] (Marburg: N. G. Elwert Verlag. 1968. Pp. vii, 302. Cloth DM 37, paper DM 31.)

EARLY modern German economic history cannot be profitably studied on a “national” level, so that any monograph presenting the results of exhaustive research in local economic history adds immensely useful data and contributes to making a more general synthesis possible. Ottfried Dascher’s history of the Hesse-Kassel textile industry is significant both for that reason and because the author has covered the entire early modern period as well as the decline of handicrafts in the nineteenth century. His account is based on exhaustive archival research and includes the use of

state commercial documents not previously organized for study by the Hessian archive at Marburg. A significant amount of statistical information has also been culled from the records and appended to the text. The narrative sometimes suffers, however, because it adheres too closely to the more fragmentary documents on privileged manufacturers, most of whom failed.

It is clear from Dascher's account that in Hesse, as in the rest of Germany, handicraft production remained characteristic from the sixteenth to the nineteenth century. The establishment of privileged manufactures (early factories) became important only in the eighteenth century, and few succeeded. The woolen industry, the leading one in the sixteenth century, declined steadily and remained chronically depressed, as happened elsewhere in Europe. During the eighteenth century much of the latter-day mercantilism of the absolute state was directed at protecting this as well as other native manufactures that had ceased to be viable or were not yet competitive. More than one-half of the woolen clothmakers earned below-subsistence incomes during the eighteenth century although more than one-half of the state's tax revenues were spent on clothing the army. The boom of the American Revolutionary War was short-lived and did not help the woolen industry. Linen became the leading textile in the seventeenth to the nineteenth century, and cotton gained some importance during the eighteenth century. The linen industry had recovered quickly from the Thirty Years' War, but experienced a depression during the 1730's and 1740's and again during the Napoleonic occupation, which contributed to destroying most of the industrial gains of the previous century. Only the *Zollverein* in 1831 once again improved the marketing of Hessian textiles, and by the 1840's even this last bit of prosperity collapsed before the flood of English machine-made goods.

In general, the author's conclusions coincide with those of other recent researchers on manufactures in the German territories in this period: the state-supported enterprises tended to fail; the bureaucracy as a class provided a large proportion of the capital for early industries; between 50 per cent to 66 per cent of the industrial population lived either on the margin of subsistence or below it; despite boom periods and higher wages in new industries (cotton), poverty remained a major problem for government policy makers.

University of Alberta

HELEN P. LIEBEL

SOCIÉTÉS ET MENTALITÉS À MAYENCE DANS LA SECONDE MOITIÉ DU XVIII^e SIÈCLE. By *F. G. Dreyfus*. (Paris: Librairie Armand Colin. 1968. Pp. 616.)

IN the 1780's Mainz had 28,000 inhabitants. It was the capital of an electoral state consisting of 16 islands in a sea of *Kleinstaaterei*, with a total area of 2,400 square miles and a population of 467,000. For Dreyfus and other historians, Mainz is significant as the city where, in 1792, the armies of revolutionary France established a beachhead in Germany and a new era of German history began. His book is a study of Mainz as a community and of the electoral state in the eighteenth century. It ends with an account of how the people of Mainz reacted to the French Revolution and the arrival of the French troops. Except for a small group of intellectuals, they reacted with hostility.

This study brilliantly exemplifies the methods of social history developed by Labrousse, Braudel, Goubert, the Sixth Section of the *École Pratique des Hautes Études*, and the contributors to the *Annales*. As such, it is a school of method. But it is more

than a socioeconomic study because it involves all aspects of eighteenth-century Mainz: political and religious institutions, finance and administration, economic conditions and growth, the progressive economic policies of the Electors, social classes and structures, demographic change, architecture and *décor*, religious minorities, immigrant groups, education, the arts, the press, intellectual activities, the German Enlightenment, and the movement of ideas. In every chapter, moreover, Dreyfus reaches out to compare the situation of Mainz with that of other Rhineland and German towns, including Trier, Frankfurt, Würzburg, Leipzig, and Strasbourg. He describes the policies of the Electors in regard to the Empire and the papacy. His chapter on the German Enlightenment is one of the most rewarding of the book and corrects French and Anglo-American tendencies to subsume it under what might be called the Enlightenment of the Atlantic world. From this point of view, Dreyfus' book approaches the character of a synthesis on western Germany in the late eighteenth century, and his bibliography is an excellent directory of the literature in German, French, and English.

To discuss the significance of Dreyfus' findings or his methodology, which at many points is fascinating, is impossible in a brief review. Among other things, he establishes that whatever was progressive in this little, aristocratic state emanated from the electoral government, which promoted toleration, and from the university, which owed its growth and advancement entirely to the archbishop-Electors. Dreyfus has been studying the history of the Rhineland since 1956. For this work he has ably exploited the archives of Mainz, Munich, Würzburg, Koblenz, Trier, Paris, Strasbourg, and Potsdam. The result is a book that will count as a landmark in the literature on western Germany in the eighteenth century and will be read with considerable profit by anyone interested in eighteenth-century Europe and the revolutionary period.

University of North Carolina, Chapel Hill

GEORGE V. TAYLOR

KATHOLISCHE UND LUTHERISCHE IRENIKER: UNTER BESONDERER BERÜCKSICHTIGUNG DES 19. JAHRHUNDERTS. By *Manfred P. Fleischer*. [Veröffentlichungen der Gesellschaft für Geistesgeschichte, Number 4.] (Göttingen: Musterschmidt-Verlag. 1968. Pp. 298. DM 35.)

IN their standard history of the ecumenical movement, Bishop Stephen Neill and Miss Rouse devoted little space to the German theologians of the eighteenth and nineteenth centuries. Only a minority of these men were the upholders of an ecumenical and irenic view of the Church. The majority of Lutheran, Reformed, and Catholic thinkers were still too dominated by the thinking of the sixteenth century to listen to the handful who pleaded for the Augustinian formula: *in necessariis unitas, in non necessariis libertas, in utrisque caritas*. Professor Fleischer has attempted to rescue the ideas and characters of those men on both the Lutheran and the Catholic side, who strove to build bridges between the rival denominations. Most of them were doomed to disappointment. Often suspected as traitors by their own denomination, they were liable to be rapturously welcomed as incipient converts by the other. In order to avoid this dilemma, some theologians withdrew into a private world, believing that they belonged to the invisible church without taking part in its visible life. Others attempted an unsatisfactory syncretism. The political rivalries of Germany, which had played such a role in sponsoring the Reformation, still played their part in maintaining denominational separatism. And while the re-

vival of German romantic nationalism in the nineteenth century lent strength to the ideal of a single German church, the anti-Catholic traditions proved too strong, and German unification under Prussia was the signal for an intensification of denominational and political ill will.

Fleischer has chosen to describe the efforts of the ecumenical-minded minority through a series of biographical sketches. This is not the most satisfactory manner of dealing with the topic because the purely biographical details interrupt the discussion of theological ideas. He lists the careers and writings of the men who supported what at that time appeared to be a lost cause. It is helpful, however, to be reminded that there were throughout certain Catholics, priests and laymen, who responded to the ecumenical imperative, and who are seldom mentioned in Catholic histories.

Bismarck attempted to unite all Germans against the "ultramontanism" of Rome, and Hitler attempted to root out the "Jewish-Christian pest" by replacing it with a Germanic religion of blood and soil. Since 1945 a new climate that offers a far truer prospect for uniting Lutherans and Catholics has appeared in Germany. Fleischer's tribute to the efforts of earlier generations is appropriately timed.

University of British Columbia

JOHN S. CONWAY

BEITRÄGE ZU EINER GESCHICHTE DES RADIKALEN NATIONALISMUS IN DER WILHELMINISCHEN ÄRA 1890-1909: DIE ENTSTEHUNG DES RADIKALEN NATIONALISMUS, SEINE EINFLUSSNAHME AUF DIE INNERE UND ÄUSSERE POLITIK DES DEUTSCHEN REICHES UND DIE STELLUNG VON REGIERUNG UND REICHSTAG ZU SEINER POLITISCHEN UND PUBLIZISTISCHEN AKTIVITÄT. By *Konrad Schilling*. (Cologne: Gouder u. Hansen. 1968. Pp. 707.)

This study is a doctor's thesis printed by offset from reduced photographs of the original typescript and bound in paper. It is, therefore, hard to read, and it is very long, detailed, and full of quotations. Since it deals with a rather general, nebulous concept, however, the richness of the quoted material is its chief value. Schilling intends to follow this with a second volume. This one deals with the *Alldeutscher Verband*, the *Flottenverein*, and government press policy in relation to both domestic and foreign affairs. The book describes and documents in detail the relationship between the bureaucracy and the organized pressure groups of the radical Right. Schilling's conclusion is that, although radical nationalism was by no means peculiar to Germany, the German political system created especially favorable conditions for its development. He thus re-enforces other recent interpretations of Wilhelminian Germany as a period when a general feeling of frustration and reckless irritability was caused by a failure of the power structure to create any consistent or progressive policy. Again, Bernhard von Bülow is the chief villain: the supposedly responsible statesman who, at the head of the governmental structure in the most critical period, 1897-1909, was incapable of developing any responsible policy.

It is useful to have all this material on the nationalist groups (with the notable exception of the *Bund der Landwirte*) brought together in one source. Yet this is also the book's major weakness: since a detailed political analysis of the period has, for the most part, not yet been worked out, Schilling relies for his political frame of reference on general secondary works such as Ziekursch, Schoeps, and Schieder. Consequently, in spite of the primary material, the movements as a whole are dealt

with on a high level of abstraction, which is accentuated by the chronological skipping about necessitated by a topical organization. Thus Schilling suspects that the political purposes of the *Alldeutscher Verband*, which was top-heavy with weighty party leaders, were probably more important than their nationalistic agitation and were served by it, but he does not know what those purposes were.

The book is based on work in the archives of the Foreign Office in Bonn, the *Bundesarchiv* in Koblenz, and the *Deutsches Zentralarchiv* in Potsdam and Merseburg. References are sometimes annoyingly cryptic, incomplete, or erroneous; many titles appear in the notes that are not listed in the bibliography, and the latter contains no non-German works on the Wilhelminian period of the last thirty years.

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J. ALDEN NICHOLS

WISSENSCHAFT UND KRIEGSMORAL: DIE DEUTSCHEN HOCHSCHULLEHRER UND DIE POLITISCHEN GRUNDFRAGEN DES ERSTEN WELTKRIEGES. By *Klaus Schwabe*. (Göttingen: Musterschmidt-Verlag. 1969. Pp. 302. DM 38.)

Two hundred pages of text and thirteen hundred footnotes suggest the conclusion that professors are people, or, to be more precise, that German professors during World War I were. Some were moderates, some were annexationists, and some held a variety of views to the left, the right, and the center of these positions. Some favored domestic reform, some opposed it, and some held various opinions in between. Some liked Bethmann, and some did not. Some were well informed about the implications of specific actions they were advocating, or opposing, such as unrestricted submarine war, and some were not. Some changed their minds as they looked at changing facts, and some did not. Some had reason for quiet pride as they looked back on their wartime pronouncements (Hans Delbrück thought Belgian annexation plans "wholly inadmissible": "Ranke divides all politicians into two groups, those who consider the gains of the moment and those who think of the future—to have a warm patriotic heart is a fine thing, but in politics it is not enough"), and some did not (Johann Plenge, economist at Münster, "A Hegel quotation shows the historical necessity of having a war of defense turn, in an hour of dire national need, into a war of conquest").

In other words, German professors were not very much different, in their attitudes toward the war, from German candlestickmakers or from German chimney sweeps, though their vocabularies were richer and their sentences longer, and they tended to sign more manifestoes. Nor were they, one supposes, very different from their colleagues in Britain, France, Serbia, Austria, Russia, or Brazil. There is, of course, no overwhelming reason to assume that a professor should be particularly worth listening to when making pronouncements outside his field. Why should a theologian from Halle be an authority on Woodrow Wilson, or a biologist from Jena be terribly illuminating about the future of Poland? But, in that case, why turn a ten-year-old dissertation into a book, especially since its author is honest enough, at the end, to say quite expressly what has long been evident to the reader: that his topic has turned out to be a nontopic; that there really was no such thing as a German community of scholars between 1914 and 1918, with certain well-defined and coherent views toward the issues raised by the war that distinguished them as a group. ("One really cannot speak of any characteristic contribution of Germany's professors toward the World War discussion . . . though one might talk about orig-

inal and characteristic contributions of *individual* scholars.”) Perhaps it is to prove that, while some dissertations can stand the transition to book form, some cannot, and some are somewhere in between.

University of California, Santa Barbara

JOACHIM REMAK

DIE SOZIALDEMOKRATIE IN BREMEN WÄHREND DES ERSTEN WELT-KRIEGES. By *Erhard Lucas*. [Bremer Veröffentlichungen zur Zeitgeschichte, Number 3.] (Bremen: Carl Schünemann Verlag. 1969. Pp. 134.)

THIS well-researched essay on the Social Democratic party in Bremen during the First World War is excerpted from a doctoral dissertation, and treats a notable case in the annals of the SPD. Here the left-wing Social Democrats were secure in their control of the party organization and membership in the prewar years, but lost their grip during the war. Much of this story centers around Alfred Henke, a left-centrist leader and journalist, and Johann Knief, a radical leftist, who struggled, sometimes together but often separately, against the SPD executive committee in Berlin over the editorial policies of the *Bremen Bürger-Zeitung*. By the end of 1916 both were defeated, for Friedrich Ebert, an earlier product of the Bremen party and now at the head of the SPD, exploited the weaknesses and division of the left-wing faction in Bremen and imposed majoritarian policies supporting war credits upon the local party organ.

Using local archival and published sources to good advantage (some of which are reproduced in facsimiles and supplemented by contemporary photographs and cartoons), Lucas delineates the role of personalities in the Bremen party group with considerable skill, but gives only very scanty attention to organizational matters. His bibliography does not mention the studies of Berlau and Ryder that might have helped to set the Bremen party policies in a broader national context. The reader feels somewhat disappointed at the end of the essay when the late war episodes of internal party history are not more clearly related to the over-all perspective of the study, but are left to conclude inconclusively.

Raymond College, University of the Pacific

GEORGE P. BLUM

SEECKT. By *Hans Meier-Welcker*. (Frankfurt am Main: Bernard & Graefe Verlag für Wehrwesen. 1967. Pp. 744. DM 78.)

GENERAL HANS von Seeckt was one of twentieth-century Germany's more interesting anachronisms. A cultivated officer and aristocrat, Seeckt began his remarkable career in the exclusive Emperor Alexander Guard Regiment Number 1 in 1885 and concluded it in the service of General Chiang Kai-shek as military adviser in 1934-1936. The most notable years of his career were between 1914 and 1926. During the First World War he played a major role in the eastern and Balkan campaigns of 1915-1916 and concluded his combat career as chief of staff to the Ottoman Army in the Near Eastern theater. Seeckt has attracted greatest attention, however, for his work as head of the *Reichswehr* between 1920 and 1926. Both the achievements and the defects of the *Reichswehr* have been associated with his leadership. He has been credited with laying the foundations of Germany's military revival and criticized for infusing the new army with the spirit of the old and for creating an army whose loyalty to the Weimar Republic was problematic.

Seeckt's first biographer, Friedrich von Rabenau, sought to assure Seeckt's position

in the Nazi era by overemphasizing Seeckt's antirepublicanism and contributions to the birth of Hitler's *Wehrmacht*. More recently, Seeckt has received very favorable treatment in Harold J. Gordon, Jr.'s, study of the *Reichswehr* and highly critical evaluations in the studies of Wolfgang Sauer and F. L. Carsten. Meier-Welcker's interpretation in this massive biography lies somewhere in the middle of these varying evaluations. The author seeks to correct Rabenau's distortions and is far more critical of Seeckt than Gordon. At the same time, he is more sympathetic to Seeckt than either Sauer or Carsten, both of whom, he argues, overemphasize Seeckt's political power and antirepublicanism.

Meier-Welcker has made an important contribution to German military and political history by writing this biography, but one wonders how many of today's overburdened scholars will have the time or the *Sitzfleisch* to get through so many pages of small print. While it is possible to agree with the author's contention that Seeckt the man must be understood in terms of his entire life, it is difficult to understand why the reader must receive detailed information concerning the various maneuvers, spas, and tourist attractions graced by Seeckt's presence. Unfortunately such digressions frequently interrupt the author's discussion of interesting and important topics. The great virtue of the author's rather old-fashioned conception of biography is that it provides a grand tour through Seeckt's enormous *Nachlass*, but this advantage is frequently bought at the loss of the reader's patience. Nevertheless, anyone interested in the German role in the Balkans and the Ottoman Empire during World War I will find a wealth of material and insight in the first part of the book. The second and largest section of the book, which deals with the 1918-1926 period, is indispensable for students of the military and foreign policy problems of the Weimar Republic. Finally, the concluding section of the book provides interesting information on Seeckt's work as a *Reichstag* deputy and important material on the role of German officers in advising the Chinese Nationalist Army.

Meier-Welcker's evaluation of Seeckt's role in the Weimar Republic is more stimulating than convincing. The author strives so hard to be fair to both Seeckt and his critics that he tends to be contradictory. On the one hand, he argues that Seeckt served the republic by maintaining the stability and independence of the army. On the other hand, Meier-Welcker recognizes that the republic needed a republican army and that Seeckt prevented the republicanization of the army. Similarly, the author denies that Seeckt wanted power, but admits that Seeckt entertained presidential aspirations and was reluctant to disappoint all the hopes that conservative circles placed in him. While admitting that Seeckt had considerable authority, Meier-Welcker denies that Seeckt was a real power in the republic by arguing that Seeckt's position depended on the support of President Ebert and Defense Minister Gessler. When the former died and the latter deserted him, Seeckt fell. One wonders if the author would apply the same line of reasoning to Bismarck because of the Iron Chancellor's dependence on the Kaiser. Seeckt was no Bismarck, but he was more than an "authority." Perhaps Meier-Welcker's interpretation would have been less paradoxical if he had developed one of the undeveloped points of his study, namely, that a full understanding of Seeckt's role requires an understanding of the conditions of the Weimar Republic which defined both the extent and the limits of Seeckt's power.

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GERALD D. FELDMAN

AKTEN ZUR DEUTSCHEN AUSWÄRTIGEN POLITIK, 1918-1945, AUS DEM ARCHIV DES AUSWÄRTIGEN AMTS. Series B, 1925-1933. Volume III, DEUTSCHLANDS BEZIEHUNGEN ZU SÜD- UND SÜDOSTEUROPA, SKANDINAVIEN, DEN NIEDERLANDEN UND ZU DEN AUSSEREUROPEISCHEN STAATEN, DEZEMBER 1925 BIS DEZEMBER 1926. (Göttingen: Vandenhoeck & Ruprecht. 1968. Pp. xlviii, 560. DM 39.)

So absorbing was the course of German relations with both Russia and Western Europe in the year after Locarno that the other concerns of German policy in Europe and the world tend to remain in the background. This volume in the new "Series B" of *Documents on German Foreign Policy*, the fifth and last concerned with the year from December 1925 to December 1926 (the preceding two volumes each appeared in two parts), rounds out the story. It is a wonderful grab bag of varied topics, but, as always, the documents are sensibly chosen and impeccably edited.

Because there are so many different subjects, continuing themes do not emerge here as sharply as in the preceding volumes. The most extensive single topic is Italy, where nearly one-third of the total number of documents is devoted to aspects of German relations with Rome. One troublesome problem was the South Tirol, linked, of course, with Austrian affairs, for which there is extensive documentation, particularly of the difficulties of January and February 1926. A second matter was the negotiation of an arbitration treaty. And, finally, the general policy of Italy with respect to France and Germany tended to be unstable. On the one hand, Italy toyed with a French-Yugoslav alliance; on the other, Mussolini protested that he wanted better relations with Germany, so much so, indeed, that the German ambassador, Von Neurath, once told him, in effect, to stop talking and start acting.

Space permits only a few brief examples of the topics to be found in the remainder of the volume. Considerable attention is given to the German efforts to regain sequestered assets from the United States. Interesting negotiations with Turkey concerned economic development and the role of the *Junkers* firm. A report of discussions between Stresemann and Austrian Chancellor Ramek in March 1926 provides illuminating insight into the problems of Austro-German relations, especially the ever-present topic of *Anschluss*, and Austria's position in Central and Eastern Europe. An interview with Beneš in September explores all aspects of Czech policy, including the problem of the Sudeten Germans. Stresemann's belief that Germany should accede to the Washington Treaty of 1922 brought sparks from Brockdorff-Rantzau and complaints from the Chinese. Germany's entrance into the League of Nations, and its assumption of a permanent seat in the Council, resulted in special pressures from Spain and Brazil, where the Germans had to walk on eggs neither to support, nor openly to abandon, the League ambitions of both nations.

These are but a few samples of the varied negotiations to be found in this volume. As always, the search for materials is aided by the excellent analytical guide to the chronologically organized documents; the student of German affairs should not miss the wide-ranging perspectives that this collection provides.

Georgetown University

THOMAS T. HELDE

LE SYSTÈME CONCENTRATIONNAIRE NAZI (1933-1945). By Olga Wormser-Migot. [Publications de la Faculté des Lettres et Sciences humaines de Paris-Sorbonne. Series "Recherches," Number 39.] (Paris: Presses Universitaires de France. 1968. Pp. 660, vii. 50 fr.)

THIS work presents a historical survey of the system established for the operation of Nazi concentration camps, and it emphasizes the origins and subsequent evolution of the system. The author, whose research acquaintance with the Nazi concentration camps began during the war when she helped collate information concerning French prisoners of war and deportees who were or had been in them, has published extensively on different aspects of the camps for several years. While possessed of vast knowledge on the subject, Wormser-Migot has nevertheless not written a comprehensive history of the camp system. The author is apparently unwilling to synthesize the abundance of detail available to her; her approach is more encyclopedic than analytical or interpretive. There is much valuable information in this book but not a coherent, unified resolution of its basic objective, which was, if somewhat hazily limited, an explication of the "why" surrounding the system's origins and the continuation and elaboration of the "how" within it in terms of function and evolution.

Within this objective the author is most successful in depicting how the system evolved and operated. Her account may be separated into two evolutionary phases. The initial stage occurred during 1933-1939 when the camps were "national" in composition, containing mostly Germans and functioning primarily but not entirely as abodes for incarceration. In the second period, 1939-1945, the camps became "internationalized" with the influx of nationalities from all parts of Europe. The latter phase also witnessed an expansion in function as the camps were operated for the multiple purpose of providing cheap labor for the war economy, furnishing human subjects for medical experimentation, and often substituting as killing centers in an extermination program paralleling the one involving the Jews in "death camps," but directed in this instance toward other groups considered undesirable, dangerous, or economically unproductive.

The description of this evolutionary process complements the account contained in Joseph Billig's *L'hittérisme et la système concentrationnaire* (1967), while it is richer in detail but not as impressive on the role of the SS as Martin Broszat's essay, "The Concentration Camps, 1933-1945," in *Anatomie des SS Staates*, by Hans Buchheim *et al.* (1965). Especially praiseworthy is the author's account of the part played by camp labor in the Nazi economy. Furthermore, her elaboration of the hierarchy of camp society in terms of the organizational structure of both the victims and the victimizers is excellent.

The major shortcomings of this work stem from Wormser-Migot's attempt to answer the "why" behind the camp system. She recognizes that the SS dominated it, and her account of the legal and illegal machinations which resulted in that dominance is comprehensive. Yet the analysis too often depicts the SS as merely the executors of a policy predetermined by the ideological stance assumed by Hitler and other Nazi ideologues rather than as the practical and ideological innovators they really were. This approach camouflages the internal dynamism of the SS and fails to demonstrate effectively that the camp system was both a means for the SS to justify its existence and an opportunity to perpetuate that existence through utilization of the camps to construct an empire.

Wormser-Migot does describe Himmler's methods in manipulating the camps

for the economic benefit of the SS, but, conversely, she does not emphasize the extent to which other components of the SS "leviathan" affected the camp system. Before the "why" or the "how" of the camps can be adequately understood, we need to know what impact the demands of SS organizations and projects, such as the *Waffen-SS* and the resettlement program of SS population policy, had on the system in terms of budgetary allocation or personnel availability. Had the author fully utilized available captured German records, especially those relating to the SS, these decisive influences might have been demonstrated.

The study rests primarily on documentation from the International Red Cross Tracing Service at Arolsen, the *Deutsches Zentralarchiv* at Potsdam, and the Nuremberg Trial records. Supplementary materials were utilized from archives throughout Europe and in the Soviet Union as well as from records possessed by various international associations of former concentration camp members, deportees, and national resistance groups. Some pertinent monographic studies were not, however, consulted, such as Enno Georg's *Die wirtschaftlichen Unternehmungen der SS* (1963), and numerous typographical errors in German both in the bibliography and in the text detract from the quality of the work. The lengthy bibliography, the inclusion of translated documents, and the excellent chronological table, together with the encyclopedic detail of its contents, make this book valuable primarily as a reference work.

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LARRY V. THOMPSON

DEUTSCHE BRIEFE 1934-1938: EIN BLATT DER KATHOLISCHEN EMI-GRATION. Volume I, 1934-1935; Volume II, 1936-1938. Edited by *Heinz Hürten*. [Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series A, Quellen, Numbers 6 and 7.] (Mainz: Matthias-Grünwald-Verlag. 1969. Pp. li, 733; 1186.)

THESE two volumes are a complete edition of the heretofore little-known German exiles' newsletter of the 1930's, the *Deutsche Briefe*. They represent part of a continuing effort by the Commission on Contemporary History of the Catholic Academy of Bavaria to present a history of modern German Catholicism. The present work is concerned with the opposition voice of those lay Catholics who left Germany after 1933. The result is a unique and valuable contribution to the history of German political exiles.

The *Deutsche Briefe* appeared in Lucerne on a weekly basis from 1934 to 1938, when it ceased publication. It was not the organ of any group or party but rather was the sole work of, and was largely financed by, two Catholic publicists, Waldemar Gurion and Otto Knab. The paper's political leanings might best be described as democratic moderate-conservative, and its primary purpose, in the view of its editors, was complete and radical opposition to the National Socialist regime on moral and religious grounds. The focus here is thus on Catholic attitudes to the new Germany. The importance of the *Briefe* derives not so much from the newsletter's somewhat limited circulation, but from the quality of its commentary and its well-informed judgments on the course of events in the Third *Reich*.

Quite unlike those Catholics who, in the early years, were busily asserting the compatibility between Catholicism and National Socialism, the paper insisted from the very beginning that there could be no compromise with a regime whose demands were total and, ultimately, totally evil. It is not surprising, therefore, that one encounters

frequent and sometimes severe criticism of those Catholics in Germany, including the hierarchy, who sought to temporize and equivocate, those who felt that the safest strategy was to ride the crest of the national revolution in the hope that it would eventually subside. Perhaps the most extraordinary illustration of the paper's stance occurred in 1935 when the bishops were exhorted to renew their earlier ban on Catholic membership in the NSDAP, and, in particular, to prohibit Catholic youth from joining the *Hitlerjugend*. The *Deutsche Briefe* also recognized at an early date that National Socialism was not simply a German misfortune. Political nihilism and *Volks egoismus* would ultimately spell disaster for all of Europe. The issues at the time of the *Anschluss* are well worth reading in this regard.

These volumes shed light on a number of crucial problems: the moral and political implications of resistance, the role of the "other Germany," and the Catholic Church's relationship to National Socialism. No final answers are provided here on the question (still very lively) of open resistance to the regime by the Church versus a policy of artful dodging. But there is much material for the debate.

As is customary with this series, a fine and very thorough job of editing has been done, with an excellent introduction, notes, bibliography, and index.

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THOMAS A. KNAPP

THE HABSBURG EMPIRE, 1790-1918. By C. A. Macartney. (New York: Macmillan Company. 1969. Pp. xiv, 886. \$14.95.)

IN his new work Professor Macartney, a respected authority on Eastern Central Europe in the English-speaking world, for the first time discusses the Habsburg Empire as a whole. His history of *The Habsburg Empire, 1790-1918*, is still far from comprehensive since cultural developments are not included, except for brief references. This may be regrettable, but it is sensible since a mere listing of outstanding achievements cannot replace analysis which, even in a volume of nearly nine hundred pages, is not quite manageable. It is more questionable whether domestic history can be separated from foreign relations, which Macartney does to a degree by cutting down diplomatic history to the barest factual minimum. There are phases in modern Austrian history, particularly in the 1860's and again during the world war period, when domestic and foreign policy is really an inseparable entity that has two facets, both of which require equal attention.

It is true, of course, that Macartney's *expertise* is anchored in social-economic history, supplemented by a fully adequate understanding of legal constitutional problems. The latter is, indeed, his first major theme. As he sees it, the history of the Habsburg Empire in its later days is that of retreat from Josephine centralism, beginning in 1790. This is not an arbitrary thesis; it is, however, subject to doubt. The Habsburg Empire was probably most cohesive in its structure under Maria Theresa, who, by full intent, stopped far short of full centralism. Under Francis I, the decentralized centralism of the Metternich system fared less badly than the neoabsolutism of Francis Joseph's early reign, and the era of the compromise in peacetime from 1867 to 1914, whatever its shortcomings, lasted as long as it did precisely because it arrested the retreat from centralism.

The introductory chapters, which deal with the reign of Maria Theresa and her brilliant elder sons, are geared to this problematical issue. Perhaps this adherence to a rigid frame makes them rather colorless, although the reader is provided with many notable facts of institutional history. Beginning with 1792 the narrative becomes

markedly more interesting, and the discussion of the Franciscan system of government is one of the strongest features of the work. Much worthwhile material and thought are offered also in the following chapters, although the significance of such events as the Slav Congress of 1848 in Prague or the consequences of the Badeni crisis in the 1890's and the Hungarian constitutional crisis of 1905-1906 are somewhat underrated. In some cases, characterization of leading personalities—for instance, Metternich, Francis Joseph, Francis Ferdinand—is very good, in regard to others of great significance—the younger Tisza, Foreign Ministers Berchtold and Czernin, and, above all, Emperor Charles—it is almost completely lacking. The last chapter on the war is only fragmentary and entirely devoid of conclusions. Thus, neither in the first chapters nor particularly in the last one does the author most effectively exhibit his undisputed qualifications.

There is another, more important objection to the presentation. The whole work is peppered with copious footnotes, often pertinent, sometimes digressive, occasionally brilliant, and very often sardonic in character. Frequently the notes present a running commentary to the text which is, therewith, largely reduced to a history of the hard and dry facts. On the other hand, substantive references to the literature, which ought to back up the author's often highly subjective interpretation, are rather rare. In many instances we do not learn clearly from what sources the presented wealth of statistical data is taken, be it the exorbitant size of Hungarian and Bohemian latifundia, be it the statement that for loans in Galicia "the neighbour usually charged 30-50%, the Jew 50-150%, and cases of 500% were not unknown." There is, of course, the general bibliography to which the reader may turn. It certainly comprises an impressive number of works. But, in any case, a bibliography cannot answer the justified demand for specific references in regard to various data.

Quite frequently the reader is not provided with the reasons for the author's snap judgments. Sometimes their unorthodox character is refreshing, but on balance it is rather disturbing. We are not told in what respect the moderate conservative J. M. Baernreither is rated as a German nationalist and Friedrich Adler as a gentle spirit. Oscar Jászi, rightly referred to in the bibliography as a brilliant, though somewhat controversial, historian, is branded in the text as an "essentially destructive mind." Again we do not learn why, unless the name Jakubovic, added to that of Jászi's in parentheses, offers a cue to this interpretation. Otherwise, this remark is as puzzling as the commentary on T. G. Masaryk's courageous defense of a Jewish peddler against a ritual murder charge is superfluous: namely, that almost twenty years later, during World War I, this action gained the Czech cause much Jewish support in Britain and the USA.

Less than accurate, and subjective in several respects, is also the bibliography. In regard to his over-all preference for conservative presentation, Macartney is fully within his rights; not so in some of his interpretations. He rightly sees much valuable material in *Das Nationalitätenrecht des alten Österreich*, edited by K. G. Hugelmann, but why does he not add that the interpretation of this important work is, in most of its chapters, strongly German national in character? Macartney correctly still considers P. v. Mitrofanov (quoted in the bibliography as Mitranov) as the outstanding biographer of Joseph II. But why he considers the work by V. Bibl as the best of the more recent biographies is difficult to understand. In his prime, Bibl, always a historian of strong nationalist tendencies, produced works of some merit. In his old age he succumbed completely to Hitler's doctrines and misconceived Joseph II as "a pace-maker for National Socialism."

By no means all of this falls into a negative pattern. In justice to Macartney it should be said that he has traveled some distance from earlier works, particularly one where he clearly professed his conviction of an inferiority of the Jewish spirit. Even then he could not, of course, be charged with sympathies for Nazi doctrines. More than that, it should readily be admitted that Macartney is right when he pays due attention to the imbalance of Jewish social stratification in Austria-Hungary's constitutional period as a cause for permanent friction, a question that is frequently glossed over by liberal historians in some passing remarks about anti-Semitism. It is indeed not the fully legitimate emphasis as such put on sensitive issues, but the subjective approach to complex sociological problems by intimation of personal guilt—and, in some cases, it is true, by praise—that is questionable. On the other hand, in the discussion of the nationality problems in Hungary, light and shadow between the policies of the various ethnic groups seem to me more equally distributed than in any of the author's previous significant contributions to the subject.

Thus, the problematical character of the book in some respects does not impair its great value in others. What does impair it to some extent are flaws in the scholarly apparatus, the unevenness as to in-depth treatment of several chapters, the rather substantial number of misspellings, errors as to names, bibliographical references, and so forth. One has the distinct feeling that Macartney could have published a much better book if he had allowed himself another six months for its preparation. In that case, the work would still have been partly controversial, but fully distinguished. Fortunately, a sizable portion of this distinction remains unimpaired.

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ROBERT A. KANN

THE LAST HABSBURG. By *Gordon Brook-Shepherd*. (New York: Weybright and Talley. 1968. Pp. xiii, 358. \$10.00.)

Mr. Brook-Shepherd has already produced four popular works on Austrian and Central European history. In this work he attempts to "retouch" and "rehang," as he puts it, the conventional picture of Emperor Charles as a well-intentioned but weak ruler dominated by his Bourbon wife. This picture, he feels, is unjust. He believes that the Emperor was a "philosopher-king" who possessed superior political capacities and moral courage and who, given a reasonable peace in 1917, would have preserved the Empire as a haven of liberty and a bastion of international order into the mid-twentieth century. The first half of the work stresses the boldness of the Emperor's efforts to negotiate a just peace and to reform the Constitution in a democratic direction. The second half describes the travail of the imperial family after defeat, including the Emperor's two attempts to regain the throne. The work rests on research in all the relevant archives and secondary sources, and the author has received extensive help from former Empress Zita.

For the general reader, this book provides an informed and absorbing narrative of the Emperor's part in the high tragedy of the First World War and the end of seven hundred years of imperial and royal power. Charles occupied one of the most august positions in the world for two decisive years, and, whatever his limitations and the lack of really penetrating material about the man himself, his brief five years of political existence deserve serious study.

The specialist, unfortunately, will find much to disappoint or irritate him here. There are minor errors of fact: universal suffrage, for instance, was introduced in 1907, not 1905, and Bucovina was in Austria, not Hungary. More seriously, the

author's own evidence does not support his conclusion and suggests that the author himself is more ambivalent toward his subject than he admits—or is perhaps aware of. This impression of uncertainty cannot be explained as merely conscientious presentation of all sides of the question, and it is strengthened by the author's own use of the terms "retouch" and "rehang," which imply an almost imperceptible revision, to describe his achievement. The reader is left at the end with the suspicion that Emperor Charles was in fact a well-intentioned but weak-willed young man dominated by a fanatical, politically ignorant, and iron-willed wife.

The author has really set himself an impossible task. It is usually possible to tell if a ruler is bad quite quickly, but it takes longer than the two years given to the Emperor Charles to know if he had the stuff of a great or a good ruler. Charles was certainly not a bad ruler; his ultimate capacity must, however, remain in question, and Brook-Shepherd's occasional gushes of admiration seem overdone. It is one thing to know what must be done, another to be able to do it. The Emperor did know that he must make peace and reform the Constitution but, on the evidence here, he seems to have had only very general, unfocused ideas and no capacity whatever to carry them out.

The author has not attempted a serious analysis of the Emperor as a political figure. When, for instance, incredible mediocrities rotate as the leading officials of the Empire amid the supreme crisis of its history, it is not enough to explain their choice by saying "there were no others." The great events of the Emperor's career are presented as a morality play in which wicked politicians thwart a saintly ruler. This naïveté and knight-errantry of the author are especially regrettable in his treatment of the Emperor's two attempts to regain the Hungarian throne in 1921. At this point he invokes the concept of feudal loyalty so as to picture the regent, Admiral Horthy, as a "traitor" and betrays his own complete ignorance of the harsh political realities that necessarily dominated any Hungarian government at that time. Of the Emperor's qualities, only his piety, humility, and dignity amid disaster and suffering emerge as unquestionable.

One aspect of this work deserves special mention. The only significant new information in it stems from the communications of Empress Zita. The Empress has long been one of the most controversial aspects of the Emperor's life. She is the most partisan, and, at this date, the most fallible of sources. Brook-Shepherd has chosen to rely uncritically on her judgment and memory. The most questionable passages in the book are those dealing with crucial events that begin "The Empress recollects." Studies of the last Habsburg must ultimately come to grips with the Zita question.

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ANDREW G. WHITESIDE

SETTECENTO RIFORMATORE: DA MURATORI A BECCARIA. By *Franco Venturi*. [Biblioteca di cultura storica, Number 103.] ([Turin:] Giulio Einaudi Editore. 1969. Pp. xxiv, 768. L. 7,000.)

SAGGI E RICERCHE SUL SETTECENTO. [Istituto Italiano per gli Studi Storici.] (Naples: the Istituto. 1968. Pp. ix, 574. L. 6,000.)

SERGIO Moravia, whose magnificent *Tramonto dell'Illuminismo* has now been published, was somewhat embarrassed when he was asked, during the St. Andrews Conference of 1967, for a general survey of the Italian Enlightenment. He could name scores of monographs on particular problems, personages, and regions—the results of a whole generation of hard work that has transformed the long-neglected sette-

cento into one of the best-known periods of Italian history. But he could offer little help to his many colleagues who had barely heard of Vico and who had never looked at the *Rivista storica italiana*. Moravia would be much less embarrassed today. For Franco Venturi, the uncrowned dean of Italian historical scholarship in general and of eighteenth-century scholarship in particular, has now provided just what was needed, at least for the decades in which the philosophes flourished in France.

Venturi's book is partially a synthesis. Whenever possible, it incorporates the results of recent historical research; even those works that were not yet published are fully reported in the footnotes. It then places these results in the widest possible geographical and cultural context, as might be expected from the author of several major studies of the French Enlightenment. But the book is also original. Whenever other scholars leave a gap, Venturi introduces his own extensive reading in contemporary literature and the documents he himself has unearthed, in London and Paris as well as in Naples and Milan. The book is not completely comprehensive, to be sure. Music is omitted, art is mentioned only in passing, science is considered only as background for philosophy, and belles-lettres, whether post-Arcadian or pre-Romantic, are presented largely as an escape from the main problems of the day—as they certainly were for Alessandro Verri. But then Venturi's purpose is not to describe Italy or even Italian culture as a whole. It is rather to account for those Italians who wrote either about politics or about such subjects as law, history, ethics, or economics that had political implications. Thus the nonliterate classes are considered at length only when they emerge as active political forces, as they did during the insurrection of Genoa and the partisan war in Piedmont. Prices and population figures are given only when they appear in the writings of contemporary observers. The administration of justice is presented through the eyes of Ludovico Antonio Muratori and Cesare Beccaria. And rural conditions in the South are described in the words of Antonio Genovese.

Venturi has very good reasons for imposing these limitations. It was Italy's men of letters, he insists, not economic *conjonctures* or demographic curves, that broke the bonds of depression, despair, and immobilism in the 1730's. It was they who drew attention to the internal conditions of their respective states when the powers of Europe, after 1748, found it convenient to leave them alone for a half century. It was they who opened their country to the experiences of their contemporaries abroad and who then exported their own experiences, in scores of translations that Venturi has carefully enumerated, for the benefit of others. It was they who turned the Italy of the scholars into the Italy of the political economists—the Italy of Carantonio Broggia and Scipione Maffei into the Italy of Pietro Verri and Gaetano Filangieri. It was they, finally, who laid the foundations for the political reforms of the following decades and who then, as officials of the more advanced states, took charge of putting the reforms into practice.

Settecento riformatore, then, is obligatory reading for all historians of modern Europe. It has obliterated several long-standing myths: that cultural creativity was possible only in large states able to fight major wars, that the vitality of Italy was dependent on Savoyard expansion, and that all the positive achievements of the age were the result of pressures from outside. It has demonstrated the progressive content of Italian historical erudition, of local patriotism, and even of the noisy debates over usury and religious holidays. Above all, it has removed all possible justification, even in the United States, for any more books on the philosophes that ignore Furio Diaz and any more surveys of the Enlightenment that do not discuss the *Accademia dei Pugni*.

Saggi e ricerche, on the other hand, is of interest mainly to specialists living in Italy. It consists mostly of exercises in the kind of research that provided Venturi with his specific information: take a document, recapitulate the contents, and then explain its genesis in terms of everything previously written or read by the author. It assumes that the reader is already aware of the importance of Costantino Grimaldi's *Risposta* and Giuseppe Valetta's memorandums and that he will be willing to read through pages of quotations and long footnotes for a small modification of what has been said by Salvo Mastellone and Nicola Badaloni. Most of the essays are concerned strictly with intellectual history—not with what actually happened in Naples, that is, but with what Pietro Giannone thought was happening and what Carlo di Borbone's edicts tried to make happen. The one attempt to bypass the eighteenth-century intellectuals bogs down in a mire of *événements* (ten ships from Genoa, four ships from Livorno, and so forth), because it also tries to bypass the methods of the *Annales*. Similarly, the one attempt to put the intellectuals into a social context gets no further than Karl Marx, who, it seems, is the current authority on the physiocrats. But most of the authors were busy working on their second books in 1964, when they were summoned to a reunion in Naples. And they went there to discuss their current projects with other graduates of the *Istituto*, not to throw valuable manuscripts into the "alarming flood of historical studies" that Ernesto Sestan complains about in the preface.

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ERIC COCHRANE

IL PROBLEMA SOCIALE E IL MUTUO SOCCORSO NELLA STAMPA SENEGESE (1860–1893). Volume I; Volume II, APPENDICE. By *Arnaldo Cherubini*. [Accademia Senese degli Intronati, Monografie di storia e letteratura senese, Volume VI, Parts 1 and 2.] (Siena: the Accademia. 1967. Pp. 486; 151.)

UNLIKE previous studies of mutual-aid societies in Italy, this one does not stress political aims, but concentrates upon the origin and development of the movement. Relying upon sources found in the State Archive of Siena, the work describes local developments. Nevertheless, many of the conclusions drawn about the mutual-aid movement in Siena are applicable to the nation as a whole.

The first two chapters of the main volume provide the reader with the framework in which these organizations emerged. Chapter 1 presents an introduction to local and religious developments from 1860 to 1893, the economic problems of the area, and finally the forces acting to encourage the mutual assistance approach there. Chapter 11 concentrates upon the "social problem" as reported by the press, with special emphasis on the newspapers of the liberal bourgeoisie, the intransigent Catholics, and the emerging socialists. The author's unfortunate practice of quoting from a series of journals, coupled with his unorthodox technique of documentation, leads to some confusion.

The third and central chapter of the volume traces the activities of the Workers' Mutual-Aid Society of Siena from its inception in 1860 to the turn of the century. It was constituted originally as an organization to assure aid to members who might fall ill; additional efforts to provide adult education, aid to permanently disabled workers, old-age pensions, and some sort of assistance to the unemployed are described by the author. Finally Cherubini analyzes the futile attempts of some members to push this organization of workers and middle-class individuals into the political arena.

The last chapter treats the minor mutual-aid societies established in Siena, while

the entire second volume is an appendix containing pertinent documents and statistics. Despite the author's poor organization of the material and a style of writing that is less than lucid, these two volumes contain valuable information and important insights into a little-studied movement.

St. John's University, New York

FRANK J. COPPA

PRZEMIANY SPOŁECZNE NA ZIEMIACH ZACHODNICH [Social Transformations in the Western Territories]. By *J. Burszta et al.* Edited by *W. Markiewicz* and *P. Rybicki*. [Ziemie Zachodnie, Studia i Materiały, Number 9.] (Poznań: Instytut Zachodni. 1967. Pp. 456. Zł. 60.)

THIS is a collective effort of eleven Polish social scientists discussing various aspects in the development of the so-called western territories of Poland. This area embraces former German provinces east of the Oder-Neisse Line that were put under Polish administration by the Potsdam Agreement of 1945.

The subject matter of the study is interesting, especially to a Western reader. The problem of the Oder-Neisse Line has been discussed heretofore primarily in the context of East-West and Polish-German relations, and little attention was paid to internal developments in the area. To be sure, both interested parties, Poland and the Federal Republic, continued to stress their rights to the disputed territories, and have published numerous "scholarly" works in support of their respective claims. In the Federal Republic the various refugee organizations have been issuing pamphlets and monographs emphasizing the apparent deterioration of the area under Polish rule, while on the other side of the two rivers the Polish research centers have been refuting German criticism pointing to a supposedly rapid progress on all fronts.

This volume makes a sincere effort to get out from under the politically inspired intellectual strait jacket, and, in parts at least, it succeeds in doing so. The result is particularly gratifying in the chapters dealing with the social changes in the area in the last twenty years.

Clearly this is an extremely fertile research ground for a sociologist and a cultural anthropologist. The massive population influx from central and eastern Poland, replacing the German population expelled to the West, was bound to create innumerable problems of integration, acculturation, urbanization, economic recovery and development, and a host of others. All of these are thoroughly discussed and analyzed with considerable objectivity by some of Poland's best-known sociologists, and they represent the most valuable part of the volume.

The same, unfortunately, cannot be said about the rest of the study. The weakest area, by far, is the chapter discussing the historical background of the area. It is not only heavily biased and one sided but also highly superficial. The only indication that it was written in the mid-1960's rather than in the heyday of Stalinism is the grudging admission that from 1945 to 1956 the official Polish policy in the area was full of errors owing to the "cult of personality" imported from the Soviet Union.

The chapter discussing demographic developments also leaves much to be desired. Admittedly, the task of obtaining reliable data regarding population transfers, for example, may well be impossible, especially as most of the transfers took place right after the end of war when statistical services in both Poland and Germany were still badly disrupted. Nevertheless, in view of the fact that this particular question is probably the most important bone of contention between the Poles and the Germans,

one would expect that special care would be given to a discussion of this controversial issue. Instead, in addition to being poorly written the chapter contains facts and figures, some of which are simply inconsistent. Thus, for example, on page 92 the author states that the number of Germans left in Poland in 1957 was estimated at 65,000. On page 123, the same author puts the number of persons who emigrated from Poland to both East and West Germany in the years 1956 to 1959 at at least 264,000. The rather striking discrepancy between these two estimates is never explained, but it cannot help but put into question other estimates contained in the chapter.

Apart from these two chapters, however, the volume represents a welcome addition to the literature devoted to postwar Poland, and it should prove helpful to all those interested in this particular country which has witnessed so much confusion and change in the past quarter of the century.

University of California, Los Angeles

ANDRZEJ KORBONSKI

BALKAN TRIANGLE: BIRTH AND DECLINE OF AN ALLIANCE ACROSS
IDEOLOGICAL BOUNDARIES. By *John O. Iatrides*. [Studies in European
History, Number 12.] (The Hague: Mouton. 1968. Pp. 211. 35 gls.)

THE record of relations among Yugoslavia, Greece, and Turkey in the postwar years, with a focus on the Pact of Cooperation and the military alliance concluded in 1953 and 1954, is the subject of this well-written volume. The author attempts to analyze those forces that compelled the three countries with basically different and conflicting official ideologies, forms of government, and historical aspirations to seek closer cooperation. He also examines the decline of the feeling of solidarity shared by Ankara, Athens, and Belgrade, and properly attributes the decline of the Balkan Pact from 1955 onward to the gradual normalization of Yugoslavia's relations with the Soviet Union, which also reflected a general relaxation of tensions, and the abrupt deterioration of Greek-Turkish relations over the political future of Cyprus.

Whenever a Balkan pact or entente has come into being, it has always been the result of great tension and has always succumbed to regional differences that reappeared as soon as the external threat seemed to have receded. The Treaty of Ankara in 1953 and the Treaty of Bled in 1954 were not exceptions to this general rule. Although on paper the Balkan entente remains in existence, all three governments consider it a thing of the past.

The major contribution of the book is the author's thoughtful analysis of Yugoslavia's relations with the Soviet bloc during the decade following the Stalin-Tito break in 1948, as well as Belgrade's path to neutralism. He also ably underlines the influence of the Trieste issue on Yugoslavia's Balkan policies. In dealing with these questions and with the foreign policy of the Papagos government in Athens, his work is well documented. The book also demonstrates the ability of the three governments to enjoy a measure of freedom of action within their geographic region. The initiative for a Balkan entente on the part of Greece and Turkey did not come from their protectors in the West.

Iatrides' discussion of Turkey's role in the alliance is perhaps the weakest part of the study. While generally accurate, it is not as thoroughly analyzed and documented. The author's difficulty in acquiring and using scarce material on the Turkish side of the triangle was obviously compounded by linguistic limitations.

The book is, nevertheless, welcomed as an addition to the contemporary history of the Balkan Peninsula. Its value is enhanced by the inclusion in the appendix of the text of the Ankara and Bled Treaties and several other agreements.

Queens College, Flushing, New York

HARRY J. PSOMIADES

IZBRANNIE PROIZVEDENIIA [Selected Works]. Volumes I and II, RUSSKAIA ISTORIA S DREVNEISHIKH [Russian History from the Most Ancient Times]; Volume III, RUSSKAIA ISTORIA V SAMON SZHATOM OCHERKE [Russian History in Outline]; Volume IV, LEKTSII, STAT'I, RECHI [Lectures, Articles, Speeches]. By M. N. Pokrovskii. Edited by M. N. Tikhomirov et al. (Moscow: Izdatel'stvo "Mysl'" 1966; 1965; 1967; 1967. Pp. 725; 664; 671; 639.)

THE scholarly prestige of the work of M. N. Pokrovskii has declined in recent years. Both in the Soviet Union and abroad, his conceptual framework as well as his specific data has been superseded. Nevertheless, the recent republication of his *Selected Works* serves a useful purpose by presenting us with a readily available edition of his major survey of Russian history, *Russian History from the Most Ancient Times*, and various examples of his lesser writings. The present edition of the *Russian History*, based largely on the 1925 edition, has been enriched by an appendix indicating the author's sources—a service Pokrovskii himself failed to provide his readers.

Pokrovskii's general synthesis of Russian history, which comprises the first two volumes of the *Selected Works*, is written in terms of a simplified Marxist analysis reflecting the aspirations and necessities of the first two decades of Soviet power. Thus his conception of such a complicated phenomenon as the Time of Troubles avoids the dynastic and international aspects of that period in order to paint the era as a struggle between different classes of landowners. Similarly, his analysis of the reforms of Alexander I and the work of Michael Speranskii seeks, simplistically, to link these events to the rise of a Russian bourgeoisie, while his treatment of the Great Reforms of the mid-nineteenth century suggests that the emancipation of the serfs was a conspiracy of the state and *dvorianin* against the peasantry. The style of the work is dry and abstract. Pokrovskii achieves piquancy and flair only when attacking non-Marxist scholars.

Pokrovskii's progressive rehabilitation among the Soviets is attested by a long introduction to the *Russian History* written by O. D. Sokolov and entitled "The Development of the Historical Views of M. N. Pokrovskii." This article is notable for its admission that during the years of the cult of personality Pokrovskii's views were deliberately misrepresented. Significantly, Sokolov ends with a statement of tempered enthusiasm for Pokrovskii's services to Marxist historiography. In short, the first two volumes of the *Selected Works* serve as a document in the history of history under the Soviet regime.

Rather more interesting for the student of Russian historiography is the fourth volume of the *Works*, *Lectures, Articles, Speeches*. The variety of material herein bespeaks the multiple roles—propagandist, educator, and political activist—that Pokrovskii played in early Soviet history. The topics range from panegyrics to Lenin as a historian, through attacks on pre-Soviet scholars to discussions of the role of higher education and archival work in the struggle against the bourgeoisie. However, several pervasive themes attenuate the variety of these writings. Such themes are the parallel but independent development of Russian and West European history; the need to

relate scholarship to political aims; the weakness of bourgeois "objectivity" in historical study; and the superiority of an emerging Marxist science of history. One is struck by an old-fashioned *fin-de-siècle* materialism in Pokrovskii's thought. For example, in an article on his colleague N. A. Rozhkov we are told that Rozhkov failed to become a true Marxist because of a tendency to be too susceptible to immediate political circumstances and too hasty in making generalizations. Pokrovskii ascribes these errors to a "physiological" and "psychological-biological" peculiarity of Rozhkov's "temperament" that he did not live long enough to outgrow. One is tempted to inquire as to the proper medication for such an ideological malady. Pokrovskii himself offers no prescription. Within five years of his writing of the Rozhkov article his own views were to become suspect. Unfortunately for Pokrovskii's reputation, there was no one to offer such a bizarre explanation for his own failures as he offered for Rozhkov's. Consequently his reputation suffered an eclipse that has been lifted by the present publication of the *Selected Works*.

University of South Florida

T. P. DILKES

AN HISTORICAL GEOGRAPHY OF RUSSIA. By *W. H. Parker*. (Chicago: Aldine Publishing Company. 1969. Pp. 416. \$8.95.)

From the outset it was not clear to me what methodological approach Professor Parker subscribed to in the preparation of his study of Russia's historical geography. (Perhaps it should be pointed out that the geographic profession remains very much obsessed by questions of method, often to the neglect of content, art, or scholarship.) As the preface notes, the text is an attempt "to combine the historical geography of the historian with that of the geographer. Reconstructions of past economic and social geographies are linked by 'geographical histories' of the intervening periods." The latter is Parker's way of adding a dynamic element to the reconstruction of past geographies. Yet if one were to ask the historian, unfamiliar with the development of modern geography, what are the "geographical factors in history" he would undoubtedly think of land forms, rivers, climate, and the like—essentially the *physical* geographical elements. On the other hand, if one were to ask the geographer what he would include in his reconstruction of the past, he would consider, among other items, the way in which people made a living, the manner in which the social-economic milieu affected the appearance of the landscape, and the patterns of interaction that had taken shape. But the author gives the impression, despite numerous quotations throughout the text drawn largely from contemporary sources on the appearance of the landscape, that such a "subject," that is, landscape change, is without authority and tradition and has no justification as historical geography. Whatever one makes of this, the text is replete with contradictory, confusing, and often ridiculous statements.

The author compares, for example, tsarist external policy with Soviet and concludes that, from early times, the former was influenced by the desire to secure better access to the sea, whereas the Soviet regime has tried to adapt itself to the country's continentality "rather than struggle against it as did Russian rulers in the past." One need not enter into the argument concerning Russia's search for warm water ports to note that Parker's assertion is out of focus. It is true that substantial economic growth has taken place in Siberia, but whether this has "lessened the significance of maritime outlets and the importance of seaports" hardly makes sense. What of the development of Soviet foreign trade and aid, not to mention the fantastic

expansion in high-sea fishing facilities and naval ambition? What of the Baltic ports, the Kurils, and Port Arthur after World War II? What, finally, is the relationship between these developments and continental adaptation? The essential question, it seems to me, is not whether the Soviet Union is adapting to continentality, but whether, for good or ill, the policy of the regime is one that "relates" to the outside world. Since the war, the participation of the Soviet Union in world affairs, accompanied by a decidedly powerful maritime interest, has been one of the basic facts of life.

One must also take issue with Parker's interpretation that the Urals long prevented Russian expansion eastward. In the central part, the mountains, as Parker notes, are low, rounded, and, in the sixteenth century, were certainly forested, thus confirming that they do not form a real climatic divide. He quotes George Cressey and others to show that the Urals are no more of a continental boundary than the Chiltern Hills or the Appalachians. Yet he does not come to the conclusion that the real barrier to Muscovite penetration eastward in the sixteenth century was the existence of the Kazan khanate. After Kazan fell to Muscovy in 1552, it was only a matter of twenty-nine years before the Yermak expedition reached the eastern slopes. What subsequently delayed Russian colonization of Siberia was distance, the threat of hostile Steppe nomads, the lack of good agricultural land in the forest zone of West Siberia, serfdom, and, until the nineteenth century, governmental policy.

A geographical explanation of the Russian "preference for increasing production by extending the sown area rather than improving the yield from existing farmland" also fails to withstand scrutiny. Before the Revolution, the total sown area of the Empire grew substantially as Siberia was put to plow, but yields also improved, as Parker notes, at least in reference to sugar beets. During the Soviet period, with the major exception of cotton cultivation in the southern valleys, the regime has not seen fit to devote to agriculture the needed resources that would stimulate the *kolkhozniki* to work and produce substantially larger yields per unit area. The only alternative has been, until now, to increase crop acreage, but a very large part of that increase has come about not by penetrating virgin areas but by reducing fallow land and pastures. After all, Russian agricultural specialists had agreed by 1913 that the best lands in the Empire had been occupied and what was then needed was intensification of land use and a raising of yields, not further assimilation.

The statement that the clearing of the mixed forest zone "eventually developed strength of muscle in the Russians, which stood them in good stead when fighting their many enemies" needs no further comment.

The historian will likely find other points of interpretation with which to disagree. For the geographer, the text leaves much to be desired. Essentially, Parker has attempted too much. The text spans the period from earliest times to 1960. He might, to greater advantage, have restricted the scope of the book and attempted perhaps only a series of cross sections or reconstructions. This is not to say that the information in the text is not useful, but a more penetrating, direct, and unbiased study would have been appreciated more.

University of Washington

W. A. DOUGLAS JACKSON

THE TSARDOM OF MOSCOW, 1547-1682. In two parts. By *George Vernadsky*. [A History of Russia, Volume V.] (New Haven, Conn.: Yale University Press. 1969. Pp. xiii, 481; 482-873. \$20.00 the set.)

THIS volume is Professor Vernadsky's final contribution to the ambitious multi-volume "History of Russia" which he and the late Michael Karpovich conceived almost thirty years ago. (The remaining volumes are to be written by other scholars, under the general editorship of Cyril Black.) His five volumes in this series and his numerous other works mark Vernadsky as the most prolific student of Russian history in the United States, past or present, and he richly deserves our gratitude for the remarkable erudition and energy he has consistently brought to his writings.

This book provides the fullest treatment of its subject yet produced in English. The titles of its seven chapters—"The Foundation of Russia's Eurasian Empire"; "The Time of Troubles"; "Reconstruction, 1619-1654"; "The Ukrainian Revolution and the Union with Moscow"; "The Tsardom of All the Great, Little, and White Russias, 1654-1667"; "Russia between East and West"; and "Rhythms of History and Aspects of Culture"—suggest something of the volume's scope and emphasis. Vernadsky continues to espouse the high significance of geopolitical factors in Russian history. Accordingly, he places heavy emphasis on the expansion of Russia and on the near fulfillment in the Muscovite era of the centuries-long struggle to bring the Steppe and its peoples under control. In reading this work one gains a heightened appreciation of the importance of the Tatars, the Cossacks, and Poland-Lithuania in shaping Russia's history during this period.

On the other hand, in treating these matters the author does not always succeed in avoiding the morass of irrelevant detail. Besides, concentration on his special interests results in some imbalance—Russia's internal history is rather underplayed. Adequate attention is given to such topics as the reign of Ivan the Terrible and the Time of Troubles, and the handling of Patriarch Nikon's church reforms and his conflict with the temporal authority is unusually thorough and effective. Cultural affairs other than religion are not covered as satisfactorily as politics, and economic and social matters are rather neglected. Although it is touched on, the important question of Russia and the West as rendered by Vernadsky leaves me less than satisfied.

Based upon a forty-two-page bibliography, this work is intended to be a synthesis. The parts are not as well knit together as one might wish, however, and the author offers few new interpretations. Even if this volume is less successfully wrought than its predecessors in the series, students of Russia's Moscow period will certainly need to consult it. It is therefore most unfortunate that it has been so poorly edited. The pages abound in typographical errors, inconsistencies in spelling and misspellings, and unsatisfactory translations. The very considerable index is incomplete, with such key items as *gost'*, *Ulozhenie*, and *Posol'skii Prikaz* omitted.

University of California, San Diego

SAMUEL H. BARON

RUSSIA UNDER THE LAST TSAR. Edited by *Theofanis George Stavrou*. (Minneapolis: University of Minnesota Press. 1969. Pp. viii, 265. Cloth \$7.50, paper \$2.45.)

Russia under the Last Tsar includes an introduction by the editor and eight essays by various authors on subjects related to the main theme. The subjects cover such

topics as a general interpretation of the last reign, conservative and radical Russian thought, constitutional growth, industrialization, education and science, cultural development and foreign policy during the years 1894-1917. Each essay deserves a review in itself. To traverse these essays within the assigned space is impossible unless we limit the review to a few pertinent aspects.

There is a degree of reiteration here and there, which is understandable in the case of a collective work. But the essays are also at times provocative enough to raise some eyebrows. The character of Nicholas II has been described by many writers in the past, notably by Sir Bernard Pares, Sydney Harcave, and R. K. Massie. From all of these accounts, not to mention memoirs, emerges a threadbare character of a weak man ruling by divine right an imperial domain that extended from the German borders to the Pacific Ocean. Again and again we find this tragic coincidence in history: one of the most pallid sovereigns on the throne at a time of gravest crisis for the Empire. Furthermore, divine rule could no more be a triumphant creed. What then, pray, is the additional historical evidence expected that might reveal "new dimensions of Nicholas' personality and behavior"?

In vain does one search here for truly original interpretations. Some views impel one either to question validity or beg reasonable disagreement. For instance, what proof is there that "Great Russian nationalism" was manifested in anti-Semitism? By a stretch of interpretation the two may be related, but by no means can they be regarded as the cause and effect in Russia. Anti-Semitism was most pronounced in the Ukraine and far less in Great Russia. That Russian anti-Semitism is a subject for special study is undeniable, but it deserves serious investigation and not superficial references. To state that Russian nationalism was "largely responsible" for the Russo-Japanese War and World War I is shallow scholarship. Such generalizations tend to lessen the value of this study and leave a serious lacuna in any work.

Perhaps the most interesting essay is by Arthur Mendel on the interpretation of Imperial Russia. His thumbnail sketch of pessimist and optimist historians who dealt with the last decade of Imperial Russia shows how capriciously history can be written. It truly justifies a Russian folk saying that in skillful hands one is able to make from the same tree trunk a cross or a dirt shovel. Mendel admits that "no historian can avoid projecting his being" as he narrates history. History thus can become a potentially dangerous weapon with its uses and abuses. For this reason, if for no others, readers of history understandably turn at times into skeptics or cynics.

One of the authors, Theodore von Laue, with a sense of intellectual remorse and fear concludes differently: America the pacemaker, despite economic success and being still mortal, may yet come to realize its sense of guilt for the frightful price Russia had to pay in its "pass and surpass" economic race with America. This is a bit too much "self-projection in historical writing."

It is regrettable that other aspects touched upon by the authors cannot be analyzed here. I can only warmly commend the book with assurance that the reader will be rewarded by way of consenting to or dissenting from the various views expressed by the different authors. In either case, it is a healthy sign in historical science.

Stanford University

ANATOLE G. MAZOUR

THE ROOTS OF RUSSIAN COMMUNISM: A SOCIAL AND HISTORICAL STUDY OF RUSSIAN SOCIAL-DEMOCRACY 1898-1907. By *David Lane*. [Publications on Social History issued by the Internationaal Instituut voor Sociale Geschiedenis, Amsterdam, Number 6.] (Assen: Van Gorcum & Comp. N.V. 1969. Pp. xv, 240. 37.50 gls.)

As Richard Pipes recently noted in these pages (*AHR*, LXXIV [June 1969], 1666), no subject in the history of modern Russia has received closer scrutiny than the emergence of the Social Democratic party. To say this, of course, is not to call for a moratorium on further research. Our picture of the Russian Marxists during their formative years is by no means complete, and much additional work will be needed to fill in the gaps. Toward this end, Dr. Lane, a British sociologist, performs a valuable service. His chief aim is to analyze the membership of the party in terms of class, status, age, education, and nationality. Beyond this, he attempts to gauge the extent of popular support for the Social Democrats on the basis of the elections to the Second Duma. And finally, in what is perhaps the most novel section of his book, he studies the local party committees in St. Petersburg, Moscow, Ivanovo-Voznesensk, Tver, Ekaterinoslav, Baku, and Omsk, focusing on their social composition, their early history, their role in the Revolution of 1905, and, where such information is available, their financial support and the uses to which it was put.

What emerges is an elaborate profile of the Social Democratic party from its founding in 1898 through the 1905 Revolution. Lane finds that party membership, even before 1905, when all revolutionary groups mushroomed, was much greater than previously thought, the figure exceeding twelve thousand activists on the eve of the uprising. Despite the widespread belief that the party was composed mostly of middle-class intellectuals, says Lane, the majority were workers and about a fifth came from the gentry, though the latter comprised less than 2 per cent of the total population. The Bolsheviks attracted more peasants than the Mensheviks, and the Mensheviks, more urban dwellers, especially professionals and skilled workers (printers, railway men). The Mensheviks, as a rule, were older, more settled, better educated, and less revolutionary. Lane, moreover, brings home the fundamental significance of ethnic factors in the party schism. While the Bolsheviks were overwhelmingly Great Russians from the central and eastern provinces, only a third of the Mensheviks were of Russian stock, nearly a third being Georgians and about a fifth Jews, and concentrated along the southern and western peripheries.

Though some of these points are familiar, never have they been presented so systematically or with such a wealth of supporting evidence. Since party archives are closed to Western scholars, Lane was compelled to draw on widely scattered data in published sources: personal memoirs, minutes of congresses, party newspapers, and the like. He might have made use of the Nicolaevsky Archives at Stanford and of the archives of the Inter-University Project on the History of the Menshevik Movement at Columbia, but otherwise his research is impressive. His title, however, is misleading and a trifle melodramatic, for what he investigates is the Social Democratic party as a whole, Mensheviks and Bolsheviks receiving an equal share of attention. His style, while lackluster, is free of meaningless jargon and generally easy to follow. In addition to the text there are thirty-six charts and tables carefully drawn up with the aid of computers. Helpful summaries follow each chapter, though the conclusion, in which the author's findings are brought together and related to current sociological theories, is disappointingly brief. The book for the most part complements, rather than repeats,

earlier studies of the subject. It is a useful contribution, of interest primarily to specialists in revolutionary and working-class movements.

Queens College, Flushing, New York

PAUL AVRICH

SOCIAL THOUGHT IN THE SOVIET UNION. Edited with an introduction by Alex Simirenko. (Chicago: Quadrangle Books. 1969. Pp. 439. \$14.95.)

THE social sciences have, of all fields of intellectual endeavor, been the most highly "politicized" and prostituted in the Soviet Union, especially in the Stalin era. Given an entire generation of scholars and would-be scholars reared in the stifling conditions of Stalinism and cut off for years from professional colleagues abroad, and given the ambiguities of the post-Stalin era (with simultaneous efforts to encourage originality and competence while also maintaining the primacy of doctrine and the veto of party authorities), the revival of the social sciences was bound to be a difficult and uneven process. After some fifteen years, it is clearly in order to take stock and assess the results.

How successful the process has been depends of course very much on the observer's perspective. Compared to the Stalin era, all the fields have flourished. Judged by other yardsticks—or compared to conditions and accomplishments in the West—the results are far more modest, ranging from remarkable to sad.

Professor Alex Simirenko, a sociologist at the University of Nevada, has in this volume put together a dozen papers by different specialists, each writing about the development of his own discipline in the post-Stalin era, and the result is an instructive if uneven collection. There are, of course, numerous difficulties inherent in handling the kinds of problems the contributors discuss. For one thing, there are substantial differences among the disciplines involved, and the boundaries of the various fields are a bit fuzzy. Some, like political science, are still formally nonexistent in the eyes of Soviet authorities; others, like sociology, have been "recognized," but are narrowly circumscribed; still others, like history, are old and established disciplines.

More serious still is a basic methodological problem. If one accepts official pronouncements as indicators of intellectual sophistication and curiosity, the picture, while distinctly better than before 1953, is still dreary and dismal. But, if he goes beyond them, the observer is faced with the necessity of "reading between the lines" (or using private information) and then either risking misinterpreting and overinterpreting a scholar's esoteric signals (and, by spelling them out, he might conceivably do an injustice to the author and perhaps cause him trouble) or else missing some of the most valuable, independent, and original insights. Indeed, the ability to separate official chaff, including ritual lip service to the canons of Marxism-Leninism, from original grain requires a measure of sensitivity that the contributors have mastered, or have chosen to display, in widely varying degrees.

One of the most perceptive and balanced pieces (based on his article in the *AHR*, LXXII [Oct. 1966], 50-73) is Arthur P. Mendel's chapter on historiography, in which he not only describes some of the recent concerns of Soviet historians but also emphasizes some of the less obvious dilemmas of scholarship, including the tacit tug of war between political and professional priorities—a variant of that Leninist perennial, "Who is using whom?"

Other papers showing similar competence as well as a good "feel" for the Soviet scene include Eugene Kamenka's chapter on philosophy, Simirenko's on sociology, Bohdan R. Bociurkiw's on political science, Donald D. Barry's on law, and Stephen

Dunn's on ethnography. Even some of the latter unfortunately limit themselves to particular aspects of a discipline, usually those in which there has been the most impressive development in recent years. Some of the other papers, while essentially summaries of recent publications, are informed and instructive.

Only two or three of the chapters struck me as wide of the mark, either in unduly pressing a political viewpoint or in their sincere simplicity. Leaning over backwards is not the posture calculated to provide the most searching look at an elusive object. It renders a service neither to Soviet nor to American scholars. The least satisfactory is William M. Mandel's paper on Soviet Marxism.

I had for some time hoped for such a volume. Necessarily it will need to be updated every few years, and already the prevalent tone of cautious optimism may be out of date in light of the new "tightening up" in 1968-1969. This highlights one of the trends apparent from the entire series of papers: while the political atmosphere and degree of official tolerance are uncertain and always subject to change, what is encouraging is the improving quality of scholarship and of the scholars themselves. In virtually all the disciplines surveyed, the growth of professionalism is pointed out, showing the secular trend toward the prevalence (to use the Maoist dichotomy) of "expert" over "red." But the ambiguities of the present include the necessity of saying much that is little more than hypocrisy and "life insurance" (a point that comes across quite well) and of not working on many important topics that remain taboo (an aspect not systematically investigated), along with the success in breaking fresh, and in some cases remarkable, ground.

This volume does show persuasively—and it is a notion still strangely resisted by many Western observers—the considerable diversity of approaches and attitudes among Soviet scholars and the reality of serious and sometimes interesting debates among them.

If in these respects this collection is successful, it cannot be considered the last word on the subject. The papers do not ask similar questions, and the editor is thus unable to make a critical comparison of the state of different disciplines and to explore the reasons for the asymmetries in their progress and prospects. The book lacks any attempt to integrate the material (even to the extent done in *Science and Ideology in Soviet Society*, edited by George Fischer [1967]). (One might likewise question the inclusion of some of the fields in what is ostensibly "social thought.") Nonetheless, and despite inconsistencies and errors in names and transliteration, this is a solid and welcome contribution.

Columbia University

ALEXANDER DALLIN

Near East

IRAQ UNDER QASSEM: A POLITICAL HISTORY, 1958-1963. By Uriel Dann.
[Reuven Shiloah Research Center, Tel Aviv University.] (New York: Frederick
A. Praeger. 1969. Pp. xvi, 405. \$10.00.)

In this work, Professor Dann has attempted the task of writing the political history of events leading to the overthrow of the Iraqi monarchy on July 14, 1958, by forces under Colonel 'Abd al Karim Qassem, who then established his own republican regime and maintained it until he was murdered in February 1963 by some of his former colleagues in the original *coup*. Essentially the work is divided into four parts that treat: the *coup* of July 1958, the founding of the republic, and the nationalist challenge;

the Communist threat and challenge to the new republican regime under Qassem; the constitutional challenge to the regime; and the end of the Qassem regime and the unceremonious departure of the "sole leader."

Dann observes in his introduction to the book that there is neither an Iraqi nation, a judgment some would challenge, nor any real tradition of cooperation to cement the various religious and ethnic communities within the country. The book is valuable in delineating the character of the various leaders, concerning whom he provides essential biographical data. It also portrays the various programs of party groups, however meaningless at times, such as those of the Communist, the Constitutional, and the National Democratic parties. There is also useful discussion of the Kurdish problem and of the problems of Arab "unity," especially in connection with possible "union" with the United Arab Republic under Nasser.

As an Israeli, Dann is barred from entry into and study within Iraq. He has had to rely very heavily, indeed, for his sources of information, as he states, on the contemporary press and radio, which are severely controlled by the government and used for propaganda purposes. The value of this kind of material is very limited. More background material would have enriched his account and given it much greater perspective. Despite this limitation, the Dann volume provides a useful preliminary study to a very complicated subject, and it gives something of the picture of the confusion, uncertainty, and instability that prevail in the country. Other studies will, no doubt, be forthcoming, and they should be based more on research in depth.

American University

HARRY N. HOWARD

Africa

THE ALMOHAD MOVEMENT IN NORTH AFRICA IN THE TWELFTH AND THIRTEENTH CENTURIES. By *Roger Le Tourneau*. [Princeton Studies on the Near East.] (Princeton, N.J.: Princeton University Press. 1969. Pp. viii, 127. \$5.00.)

THIS slim volume contains extensively revised versions of three lectures delivered at Princeton University in 1959, with an inaccurate map (in French!), uneven footnotes, a brief bibliography, and an index that failed me repeatedly. Original sources and subsequent scholarship are awkwardly blended into a reliable historical sketch, followed by shrewd analysis and analogical application to contemporary North Africa.

The first, and best, of the three chapters treats "The Birth of a Movement" clearly and comprehensively. Ibn-Tumart, founder of the Muwahhīd (unitarian, "Almohad") sect, is followed from his Berber origins through his studies in the Levant, his reformist phase in Tunisia, his conflict at Marrakesh with the Murābiṭ ("Almoravid") ruler, his retreat to the Atlas and unification of the Berber mountain tribes against the dominant Saharan oligarchy, and his emergence as Mahdī, to his death in 1130. A convincing appraisal of his character, theology, and legacy completes this section splendidly.

"Building an Empire" continues with a comparably thorough and satisfying treatment of Ibn-Tumart's successor, the caliph 'Abd-al-Mu'min, who forged an army, overthrew the Almoravids, conquered North Africa and much of Muslim Spain, established an efficient administration, and left distinguished architectural monuments. But he is sternly censured for naming his son Yusuf heir to his empire, thus debasing the religious movement into secular Berber imperialism. The remainder

of this chapter, covering fifty years of achievement despite rebellions and centrifugal strains, is somewhat perfunctory, but adequate.

"Decay and Collapse" is sketchy, historically, and emphasizes analysis of the causes of the empire's rapid disintegration. This, like the comments on the two founders, is the best available in English; it bears comparison with recent Spanish scholarship, which is skillfully utilized and warmly acknowledged.

Unfortunately, the striking parallels between Almohads and Almoravids—religious origins, military conquests, flourishing civilizations, rapid collapses—are scarcely mentioned, and such niceties as cartography, transliteration, copy editing, and indexing are surprisingly substandard.

Princeton, New Jersey

HARRY W. HAZARD

MOROCCO'S SAHARAN FRONTIERS. By *Frank E. Trout*. [Bibliotheca Africana Droz, Number 1.] (Geneva: Droz Publishers. 1969. Pp. 561.)

THE product of eight years' work, this volume examines Morocco's frontier in the Sahara from the Treaty of Marnia in 1845 to the present time. While it is fortunate that the author conceived his problem to be broader than the technical one of political frontiers, he does achieve that limited goal masterfully, and forty-five map plates help to make clear the message. Of greater interest to me is the attention given the nomadic tribes, the people who lived in "the empty space," their ways of life, and their needs in relationship to the Moroccan, French, and Spanish administrations. Before the coming of the French and Spanish no boundaries were necessary, and Trout notes that "for all of the period of the colonial occupation of the western Sahara, that is before 1956, there is not a single instance of a boundary, either a *de facto* or a *de jure* line, being enforced in order to make inter-territorial migrations impossible."

Another exciting theme that the study develops is the conflict in objectives and policy between the French officials in the Protectorate, particularly during Lyautey's years, and the French leadership in Algeria. Lyautey and his *équipe* defended Moroccan particularism. This took the form of extending administrative control and laying claim to territory, as, for example, Tindouf. As the colonial regime neared its end, these rivalries and conflicts diminished, and the European powers tried to secure whatever future favors they could. Soon independent Morocco's territorial appetite in the Sahara and Mauritania turned out to be exceedingly healthy.

In his conclusion Trout makes some sensible boundary suggestions and wisely does not expect them to be accepted by the newly independent states. He sees the present state of *de facto* limits with no international boundary agreements between the countries as hazardous to peace and imposing difficulties upon the nomads. They in turn by their existence and way of life keep "the issues alive."

Appearing nine years after Philippe Husson's *La question des frontières terrestres du Maroc*, this volume broadens our knowledge and updates our information. At the same time it indirectly suggests the need for more critical field study of the tribes.

Oakland University

RICHARD M. BRACE

GUBULAWAYO AND BEYOND: LETTERS AND JOURNALS OF THE EARLY JESUIT MISSIONARIES TO ZAMBESIA (1879-1887). Edited by *Michael Gelfand*. With a foreword by *W. F. Rea, S. J.* (New York: Barnes and Noble. 1969. Pp. 496. \$10.00.)

THIS is a collection of the letters and journals of the ill-fated Jesuit mission to the Zambezi region in the years just prior to the creation of the white colony of Rhodesia. It has been admirably edited by Michael Gelfand, who has not only given the necessary background and biographical information but has also contributed an interesting foreword in which, as a professor of tropical medicine, he is able to discuss the diseases that were one of the major factors in the failure of the mission. However, these letters and journals add only marginally to our knowledge of the Ndebele and other African groups among which the Jesuits moved. There are some interesting descriptions of Lobengula, perhaps most useful as a record of European reactions to the Ndebele king, but, on the whole, the accounts of African life are disappointing. This is partly because the Jesuit mission was never as important or as politically involved as the Protestant missions in this area. It is also, as Gelfand points out, because the Jesuits did not seem very interested in the comparative study of African groups, perhaps because they had little advance linguistic training. Finally the Jesuits were, in part, prevented from entering certain areas such as Barotseland by the opposition of European traders, in particular George Westbeech. As a consequence the historical record is rather meager. For the historian, the Jesuit mission is more important, not for the accounts of the period 1879-1887, but in providing a basis for the Roman Catholic missionaries who came with the Pioneer Column and afterward.

Loyola College, Montreal

DONALD C. SAVAGE

THE DIARY OF A. J. MOUNTENEY JEPHSON: EMIN PASHA RELIEF EXPEDITION, 1887-1889. Edited by *Dorothy Middleton*. With preface, prologue and epilogue compiled by the Editor in collaboration with *Maurice Denham Jephson*. [Hakluyt Society Extra Series, Number 40.] (New York: Cambridge University Press for the Society. 1969. Pp. xi, 454. \$18.50.)

IN January 1887 Henry Morton Stanley organized an expedition to march to the Upper Nile to relieve Emin Pasha, governor of the Equatorial Province of Egypt, who had been isolated in the Southern Sudan since 1882 by the forces of the Mahdi. The motives for the expedition were many. The British businessman, Sir William Mackinnon, sought to use the expedition, and Emin, to secure a sphere in Equatorial Africa for his commercial ventures. King Leopold of the Belgians hoped to extend the territory of the Congo Free State to the Upper Nile by means of the expedition. Some expected to profit from Emin's vast horde of ivory, but most Englishmen were inspired by the bizarre figure of Emin Pasha intrepidly holding the torch of civilization in the midst of Darkest Africa before the forces of barbarism that surrounded him. The British had failed to rescue Gordon at Khartoum in 1885; they could not abandon Emin Pasha in 1887. The welter of motives was matched only by the ambiguity of Emin himself. On the one hand, he had asked for relief; on the other, he remained reluctant to be rescued. He never really made it clear which he preferred, and this confusion of objectives compromised the expedition from the beginning and, upon its conclusion, cast a shadow of doubt over the results.

The Emin Pasha Relief Expedition has produced a considerable bibliography, the direct result of public interest in the expedition and the controversies generated by

its objectives, organization, and management. Unfortunately, none of the many books about the expedition have resolved the thorny questions that it created. Thus, the diary of A. J. Mounteney Jephson, one of the principal officers of the expedition, written between January 1887 and October 1889 and discovered only in 1955 in a cupboard at Mallow Castle, the Jephson ancestral home, contains a fresh firsthand account that has been edited by Dorothy Middleton and includes a prologue and an epilogue prepared in close collaboration with the late Brigadier Maurice Denham Jephson, the diarist's nephew. Although most Africanists would have preferred to see the complete diary reproduced, the editor has deleted approximately a third of the diary because it is presumably a repetition of Jephson's account given in his own book *Emin Pasha and the Rebellion at the Equator* (1890). The editing is clear and complete, the production excellent, and the price fantastic. The editor's introduction is a succinct account of the incredibly complex events that led to the expedition, but her appraisal of Robert Felkin and his interest in Equatorial Africa is much too charitable. Even Jephson found him unreliable. Regrettably, the maps are quite inadequate for the text and inappropriate for the high price of the volume.

Although his patroness, the Comtesse de Noailles, paid one thousand pounds for Jephson to join the expedition, he has always appeared as the most appealing member of it. The diary confirms this impression. It also tells us more about Stanley in particular and British attitudes and assumptions toward Africans in general. In any event, the whole tangled skein of the expedition and events in Equatorial Africa is now being examined more critically by historians of Africa, all of whom will find Jephson's diary a useful tool in unraveling the mysteries of that most remarkable expedition and the disintegration of Emin Pasha's rule in Equatoria.

University of California, Santa Barbara

ROBERT O. COLLINS

A HISTORY OF THE ASIANS IN EAST AFRICA, C. 1886 TO 1945. By J. S. Mangat. [Oxford Studies in African Affairs.] (New York: Oxford University Press. 1969. Pp. xvi, 216. \$7.75.)

It is ironical that, at the very moment when the Asian community in East Africa is undergoing its most trying test, the first scholarly treatment of its history should appear. J. S. Mangat's well-researched and carefully presented study traces the major themes of Asian history in East Africa from the early immigration before the colonial period to the end of World War II. He documents a number of points that have troubled scholars. The colonial authorities, British and German, at first welcomed Indian immigrants for the help they could provide as workers, traders, clerks, and soldiers, but this welcome was gradually withdrawn under pressure from European settlers and competition in the economic sphere. The Europeans created a myth about the Asians by constantly describing them as unfair traders and unhygienic in personal habits. The growing resentment eventually resulted in a severely stratified society, especially in Kenya. The settlers exercised predominant political power and had privileged residential and landholding rights. The Asians were dominant in the commercial sphere, especially as petty traders, while the Africans lived on reserves and worked periodically in cities and on European farms.

Mangat's essay is useful for stressing major themes and providing an overview. It is stronger on Kenya than on other parts of East Africa and on the first two decades of the twentieth century than on other periods. Unfortunately the orientation of the study is rather conventional, and key problems are not probed deeply. The essay

revolves around Asian-European relations, no doubt because these were crucial to the Asian community. But many Africanists will be disappointed that there is not more on the way in which the Indians impinged on African life. The internal organization of the numerous Indian communities is not discussed. There is very little on Islam and Hinduism in this new setting. The author mentions that the Goan and Ismaili groups were more receptive to social change than other groups, but does not elaborate on this statement. Racial prejudice is frequently mentioned, but there is no probing analysis of the roots of prejudice. It would have been too much to have expected Mangat to tackle all these issues. Many should be the subject of whole books, but perhaps a few could have been explored in more depth.

Princeton University

ROBERT L. TIGNOR

MALAWI: A POLITICAL AND ECONOMIC HISTORY. By *John G. Pike*. [Praeger Library of African Affairs.] (New York: Frederick A. Praeger. 1968. Pp. viii, 248. \$6.50.)

KENYA. By *A. Marshall MacPhee*. [Nations of the Modern World.] (New York: Frederick A. Praeger. 1968. Pp. 238. \$6.50.)

THESE two recent books on African states deal mainly with the events of the past two decades and share the assumption that in political affairs rulers and colonial elements occupy the center of the historical stage. The authors lived through much of this period in the countries they describe: John Pike employed as hydrologist in Malawi; Marshall MacPhee, a journalist in Kenya. Their backgrounds mold their presentations: Pike presents the environmental and agricultural situation of Malawi with authority; MacPhee offers a well-written if conservative synthesis of political developments in Kenya.

To treat Malawi as a discrete physical unit, as Pike chooses to do, is appropriate for the small state whose domestic concerns are dominated by the need to increase agricultural productivity and contend with a dense population. Attention to the rise of the Congress party and the paternal dictatorship of Dr. Hastings Banda is similarly justified as an important chapter in recent history. But an economic and political survey of the country is hardly complete without due reference to regional interconnections, beginning with the long-distance trading patterns of the precolonial era and continuing to this day in the form of large-scale labor migration. Political ideas returned with the workers, and missionary education also encouraged a progressive African voice. The facts simply do not support such a blanket assertion as: "The period between the end of the slave wars and the trade depression [1930's] was one of silence for the African population numbed by the changes that had overtaken them." Banda's pro-South African policies must be related to the continuing export of labor and cannot be explained solely through the fact that Malawi must export its produce through Portuguese-controlled ports.

MacPhee features the white men in the history of Kenya and seems discomfited whenever he must take account of Africans, who are referred to as "the dark people" in discussions of early times and become "the black hordes" of Mau Mau. Only when they are organized into parties following constitutional rules do some Africans emerge as real people. Notwithstanding the Africanization of the story of the last decade, broader social and economic problems remain obscure.

It seems to be inevitable that popular surveys of new African states will continue to appear. It would be well if future authors, being more aware of scholarship re-

lating to their countries, would at least amplify the old outlines of colonial historiography to achieve a more dynamic understanding of the African past.

Columbia University

MARCIA WRIGHT

THE OXFORD HISTORY OF SOUTH AFRICA. Volume I, SOUTH AFRICA TO 1870. Edited by *Monica Wilson* and *Leonard Thompson*. (New York: Oxford University Press. 1969. Pp. xviii, 502. \$8.00.)

SOUTH African history to date has been the story of European achievement. It "starts" with the arrival of the Portuguese. Nonwhites appear as "problems"; their societies, primitive and static. Bushmen, Hottentots, and Bantu have been neatly categorized as hunters, herders, pastoralists, speaking distinctive languages and occupying separate territories. South African historians have felt inhibited about cultural analysis. South African anthropologists have been equally inhibited about applying to the dominant Boer and British minorities the *expertise* they bring so fruitfully to African studies. These are the prejudices, distortions, and myths in the writing of South African history that the editors of this volume seek to correct. R. R. Inskeep, an archaeologist from Cape Town, examines evidence of finds in the republic in an opening chapter. The span of South African history is more properly ten thousand, rather than five hundred, years. All the peoples in the complex multiracial structure of South Africa have contributed to its history, and Monica Wilson, an anthropologist from Cape Town, follows with chapters on the Bushmen (San), Hottentots (Khoikhoi), Nguni, Sotho, Venda, and Tsonga. Using oral tradition, artifacts, and linguistic studies, as well as written evidence dating from the first European explorers, she demonstrates the diversity and evolving character of cultures that were never static. She explodes the myth that the nonwhites of South Africa once lived in some pristine state of apartheid. M. F. Katzen, a historian, now of Leicester, writes of the Cape under company rule, and Wilson follows with an excellent analysis of culture contact on the Cape's eastern frontier from the seventeenth century, rather than from the usual 1770's, to 1870. The thrust of her argument is announced in a subheading—"Civilization by Mingling." T. R. H. Davenport, a historian at Rhodes University, takes up the story of the Cape after the British annexation, and L. M. Thompson, a historian at Yale and formerly of Cape Town, rounds off the book with two chapters on developments in Natal and on the high veld following the rise of the Zulu kingdom and the intrusion of the trekboers. A comparison of Katzen's account of the Cape in company days with those of the Cape and Natal after 1815 prompts some reflections. Katzen has no difficulty in examining the Cape within an Asian as well as a European context. Official records require it. Decisions in Batavia affected the Cape as did those emanating from Holland. Why does this Asian background almost disappear in the discussion of the Cape and Natal after 1815? Although the focus, in this as in other histories, is almost exclusively on internal change, the Durham Report is noted as having an impact on the Cape: Sir George Grey brings New Zealand into the picture; the transportation of convicts from Australia in 1849 precipitated an explosion at the Cape. But what about India, possibly even China? There are only four brief references to India: it was a market for South African horses; it denuded the Cape's eastern frontier of German veterans of the Crimean struggle during the crisis year of 1857; it provided Natal sugar fields with indentured labor; it made an "expansionist" of Sir Harry Smith. Only in this last reference is there any hint of the tremendous events that were disturbing India at the time. The British, after all, seized

the Cape and Port Natal for reasons of naval strategy. After Trafalgar and Waterloo they exploited their naval and commercial superiority ruthlessly in both South and East Asia, and the Cape route was central to all this down to the opening of the Suez Canal in 1869. The British in India were fighting Sikhs, Gurkhas, Marathas, and Pindaris; they were, in fact, *conquering* India. They attacked Afghans and Burmese. In East Asia, British ships were buccaneering off the coast of China, crashing into the China trade with cargoes of opium. Yet, even in this reappraisal of South African history, we are again left with the impression that a rebellion in Canada, or Maori wars, or Australian convicts had a greater impact on South Africa than the mastering of India or the rude awakening of China. The same is true for slavery. In this, as in other histories, the issue is discussed as a domestic matter (with West Indian overtones); it was an issue settled in the Cape between 1807 and 1834. But the Royal Navy was fighting the slave trade off the east coast of Africa well into the 1870's. Was the southeast coast not involved? There were repercussions from Muscat to Zanzibar and Delagoa Bay. Did they not in any way penetrate Natal and the Cape? These questions may fairly be asked of a work that sets out to explore fresh angles on South African history.

The bibliography is comprehensive with one surprising omission. There is no reference to the rewriting of Natal history, in much the same spirit as this book, by Edgar Brookes and Colin Webb in 1965. Theal takes many a jab in these pages, but not a glove is laid on Hattersley who makes so many of those "misleading assumptions" about South African history that the editors deplore in his annals of Natal. Other Oxford histories have offered useful aids in the form of footnotes containing biographical data and appendixes with listings of government members. The editors have dispensed with these (there are, however, genealogies of several African ruling houses in the text). The approach throughout is sociological with an emphasis upon cultures and groups and the play of impersonal forces. South Africa is rich in individual personalities that have shaped its historical development. Their role is muted in this book. It would be captious to labor the point as this publication so significantly alters perspectives in South African history. A second volume will deal with the period after 1870.

Renison College, Waterloo, Ontario

WYN REES

Asia and the East

CHINA IN REVOLUTION: THE FIRST PHASE, 1900-1913. Edited and with an introduction by *Mary Clabaugh Wright*. (New Haven, Conn.: Yale University Press. 1968. Pp. xiii, 505. \$15.00.)

THE papers that make up this valuable study resulted from a conference sponsored by the Joint Committee on Contemporary China of the American Council of Learned Societies and the Social Science Research Council. The quality is uniformly high, and the total contribution to historical knowledge is of immeasurable value to anyone seriously interested in understanding the roots of China in revolution.

Twenty-two scholars from seven countries participated in the conference; half of their papers are contained in this volume. Dr. Mary Wright, of Yale University, has written a perceptive introduction that ties the papers together into a neat, intellectual package. She shows convincingly that the rising tide of change after 1900 was the

logical successor to the "last stand of Chinese conservatism." Using the research of her colleagues as her ammunition, she shoots down the bland interpretations of the period 1900-1913 that have been in vogue. She demonstrates that revolutionary movements that culminated after 1949 passed through a first phase at the end of the Empire, a second phase during the Kuomintang period, and a third phase engineered by the Communists. The documentation of her presentation is impressive, and the whole book is a mine of information about source materials in modern Chinese history.

Part I analyzes "Political and Ideological Movements." The papers are by Michael Gasster of the University of Washington on "Reform and Revolution in China's Political Modernization," Martin Bernal of Cambridge University on the "Triumph of Anarchism over Marxism, 1906-1907," P'eng-yüan Chang of the Academia Sinica on the "Constitutionalists," and John Fincher, also of the University of Washington on "Political Provincialism and the National Revolution." The net effect of the four papers is to expose the depth of revolutionary roots in China at the turn of the century; never again can anyone argue that the period of revolution was ushered in by the accidental bomb explosion in 1911.

Part II discusses the "Sociological Aspects of the Revolution." Marie-Claire Bergère, of Paris, deals with the role of the bourgeoisie, and Chüzō Ichiko, of Tokyo, with that of the gentry. Mary Backus Rankin, of Washington, D.C., focuses her attention on the revolutionary movement in Chekiang as an example of the tenacity of tradition.

It is clear that the traditional classes were far from monolithic, as both the bourgeoisie and the gentry had revolutionists as well as reformers and die-hards in their ranks. The authors' reasonableness plays havoc with the Chinese Communists' interpretation of the origins of revolution, as well as with standard cliché-ridden Western interpretations. They point out that the old social groups were ambivalent and that a new society of such new groups as youth, women, overseas Chinese, students, workers, and military men was already discernible by 1911. Even a significant proportion of the "people of China," including the usually inert peasant masses, were far from uninterested in the fate of China as a whole at the hands of foreigners and the problems of the remote areas within China. The leaven of nationalism was active, and the desire for modernization was genuine.

Part III is entitled "Military Power in the Genesis of the Revolution." Yoshihiro Hatano, of the University of Nagoya, writes of the "New Armies," and Vidya Prakash Dutt, of the University of Delhi, describes the "First Week of the Revolution—the Wuchang Uprising." He presents the interesting thesis that the new armies of the imperial regime were "more than agents of modernization in the upper strata of Chinese society." They had links with the peasantry, and "it was the enlisted men and officers of the lowest rank who took the lead in revolt and forced their superiors to follow." Dutt plays down the role of the Revolutionary Alliance in forwarding the revolution in 1911 and gives much credit to lesser known groups such as the Literary Institute and the Progressive Association, and to unorganized elements that spread their ideas largely through secret societies.

Part IV is devoted to the "Limitations of Revolutionary Leadership." Ernest P. Young, of the University of Michigan, writes about "Yuan Shih-k'ai's Rise to the Presidency," and Harold Z. Schiffrin, of the Hebrew University in Jerusalem, discusses the "Enigma of Sun Yat-Sen." Neither Yuan nor Sun had the stature nor the capacity to comprehend or to lead the movement, which was bigger than its leaders.

The revolution swept on through its first phase and was subsequently channeled into its later phases by the Kuomintang and the Communists.

It cannot be entirely claimed that this excellent book is solely a product of the younger generation of scholars in the China field. The influence of such pioneers in the field as John Fairbank, Arthur Wright, George Taylor, Albert Feuerwerker, Marius Jansen, and William Skinner is clearly evident. In my view, these distinguished historians together have contributed to a hypothesis that contemporary revolutionary China is deeply rooted in its own modern history, and what the world has on its hands in China is not a new dynasty but a new and distinctive country.

Stanford University

CLAUDE A. BUSS

CHINESE COMMUNIST POLITICS IN ACTION. By Roy Hofheinz, Jr., et al. Edited by A. Doak Barnett. [Studies in Chinese Government and Politics, Number 1.] (Seattle: University of Washington Press, 1969. Pp. xxvi, 620. \$12.50.)

CHOU EN-LAI: CHINA'S GRAY EMINENCE. By Kai-yu Hsu. [Anchor Books.] (Reprint; Garden City, N.Y.: Doubleday and Company, 1969. Pp. xvi, 263. \$1.75.)

Chinese Communist Politics in Action is a symposium of eleven papers originally presented at a conference on the Micro-Societal Study of the Chinese Political System sponsored in 1967 by the Joint Committee on Contemporary China of the American Council of Learned Societies and the Social Science Research Council. These papers, Doak Barnett says in the introduction, "are representative of some of the best research now being done on the political system in Communist China by a new generation of young scholars."

Covering a variety of topics ranging from a study of patterns of Communist influence in rural China, 1923-1945, to social welfare and industrial workers on the current scene, these papers, with few exceptions, purport to examine small segments of Chinese Communism using Western political and sociological theories and methodologies. It must be said at the outset that they are thoughtful and sometimes interesting essays and should contribute significantly to our knowledge of Communist China.

As the authors themselves admit, because of the paucity of firm data, the conclusions offered can only be tentative and suggestive of further hypotheses. The latter, in my view, is the chief value of the book. Although Chinese political behavior should not be regarded as *sui generis* requiring a uniquely different approach and expertise, it does not follow that all stages in the development of China's political system can be studied dogmatically in terms of Western theories. Fortunately, the authors of this volume are always careful to point out the limitations and differences.

At this stage, Chinese society itself is in flux and does not lend itself readily to conceptualization and broad generalizations. In this regard, one may therefore legitimately question the validity of looking at the quantifiable background of a small group of local and regional cadres for criteria of recruitment and degree of bureaucratization (Part II), for it is possible that at an early stage of modernization in any society job allocation is simply based on the availability of talent. Therefore the statistical data presented here may well be incidental to this developmental process. The value of these quantitative studies would, however, be enhanced if supplemented by further studies covering a longer span of time.

The volume ends with a discussion by Michel Oksenberg of sources and methodological problems in the study of contemporary China that should prove helpful to students in the field.

The volume on Chou En-lai by Kai-yu Hsu is a different genre. Based on some well-known historical sources and interviews with anonymous informants, it is a dramatized "biography" in the Chinese storytelling tradition, reminiscent of some recent anecdotal histories such as T'ang Jen's "Chin Ling Ch'un Meng" series. From its pages Chou emerges, as expected, as a central figure throughout the history of the Chinese Communist movement—pragmatic, flexible, opportunistic, urbane, and human. It is a well-written and entertaining story and little else. No real biography of Chou will be written for a long time.

Temple University

S. M. CHIU

THE MODERN HISTORY OF MONGOLIA. By C. R. Bawden. [The Praeger Asia-Africa Series.] (New York: Frederick A. Praeger. 1968. Pp. xvii, 460. \$11.50.)

THIS exceedingly learned book recounts and analyzes in considerable detail the history of the area commonly known as Outer Mongolia. It begins when the Mongols came under Chinese or, rather, Manchu rule in the seventeenth century and concludes with the most recent developments in the so-called "Mongolian People's Republic."

The author's treatment of this subject is both perceptive and objective, as well as so exhaustive that at times the reader is almost overwhelmed by the sheer quantity of information. Especially interesting and revealing is his description of perhaps the single most important event in the recent history of Mongolia, namely, Communist destruction of Lamaism, followed by the virtual extirpation of Buddhism as an organized religion among the Outer Mongols. Although staffed chiefly by parasitic, exploiting, and vice-ridden priests who were in large part responsible for the extraordinary prevalence of venereal disease among the Mongols and headed by an alcoholic Hutukhtu or "Living Buddha" so syphilitic that he was half blind, the lamaseries nevertheless commanded the pious allegiance of most Mongols. Together with collectivization of the Mongolian economy, their eradication was accomplished in the face of bitter, frequently armed, opposition on the part of the vast majority of the population. Since this resistance was suppressed largely by Soviet troops, the Communist regime in Outer Mongolia became utterly dependent on the Soviet Union, with the result that after 1933 Mongolia's history is in most respects indistinguishable from that of the USSR. According to the author, this entailed both advantages and disadvantages for the Outer Mongols. On the one hand, under Stalin's protégé Choibalsang, they suffered the same despotism and paranoid purges that the Russian dictator imposed on the Soviet Union; at the same time, they benefited enormously from extensive Soviet technical and economic assistance, especially after 1950 when Communist China's efforts to acquire influence among the Outer Mongols caused the Russians, as well as Communist states in Eastern Europe, to provide Outer Mongolia with unprecedented aid of all kinds. This aid and social and economic reforms carried out by the Communist rulers of Outer Mongolia have rapidly transformed that country from a feudal society into what might be called a modern state, capable of providing its citizens with both rising living standards and an ever-higher degree of education and sophistication. As for Outer Mongolia's relations with China, the author relates in detail the history of China's aggression against its nomadic neighbors, culminating in the merciless exploitation of the Mongols at the hands of Chinese merchants. The Outer Mongols were freed from this burden only by the intervention of the Soviet Union and the establishment of a Communist government in Outer Mongolia. In fact, it is doubtful

that, without the Soviet Union, an independent Mongol state could have survived into modern times. Consequently, the author has no doubts concerning Outer Mongolia's position with respect to the current Sino-Soviet conflict. "All material progress in Mongolia depends ultimately on the willingness of her allies to go on carrying much of the cost of her development," he maintains. "As a corollary, Mongolia will doubtless remain inside the Soviet section of the divided communist world."

I enthusiastically recommend this book to anyone interested in the revolutionary process in the so-called "backward," "underdeveloped," "emerging," or "third world" areas of the earth.

Vassar College

DONALD G. GILLIN

THE JAPANESE COMMUNIST PARTY, 1922-1945. By *George M. Beckmann* and *Okubo Genji*. (Stanford, Calif.: Stanford University Press. 1969. Pp. 453. \$12.50.)

This book is a very competent, detailed study of the prewar history of Japanese Communism, the Japanese Communist party, and the efforts of the Comintern to support, revive, and reorganize them in the face of ruthless suppression by the Japanese police. The narrative divides itself into three periods with essentially the same cycle. The first period covers the struggle, including widespread disagreement on theory and strategy, to organize the first party in July 1922, the subsequent extensive arrests of its leadership, and the party's dissolution in 1924. In the second period (1925-1929), during which the Comintern played a decisive role, the same cycle occurred. The second party was formed in 1926 and dissolved in 1929. In the final period from 1930 to 1945, Communism as a movement failed to survive the pressures of Japanese nationalism, expansionism, and war.

In covering these developments, the authors have emphasized the shifts in leadership within the Communist movement in Japan and the differences among the leaders and between them and the Comintern, especially on the theoretical and strategic basis for Japanese Communism. For example, in the Fourth Comintern's platform of 1922 and in the Theses of 1927 and 1932, Moscow argued that Japan's heritage and state of capitalistic development required a revolution in two stages, namely, a bourgeois-democratic revolution that would develop into a socialist revolution under the leadership of the Japanese Communist party. Hence, Japanese Communism must oppose imperialist wars, advocate the abolition of the monarchy and the establishment of basic political, social, and economic reforms. For their part, many Japanese theorists challenged parts of the Theses and gave their own interpretation to Communism as it applied to Japan. In the end, most of them either conformed to the Comintern's dictates or defected.

As sources, the authors have extensively used reports and records of various government ministries, the writings of the leading Japanese Communists, including the party's official publications, and extensive published works on the leftist movement. One-fourth of the text is devoted to translations of all of the important Theses, to biographical sketches of over fifty of the leading Communists, and to a special chronology. The reader, whether interested in international Communism or in this period of Japanese history, will find this volume of interest and a mine of detailed information. The study happily complements George Totten's *The Social Democratic Movement in Prewar Japan* (1966) and Robert A. Scalapino's *The Japanese Communist*

Movement 1920-1966 (1967), which devotes only the first chapter to the years prior to 1933.

The authors have clearly achieved their objective of covering Japan's prewar secret Communist movement and its double role of spreading Marxism and serving as a precursor to the postwar Japanese Communist party. They have also shown clearly how the nature of Japanese prewar society made it impossible for Communism to co-exist with it. As it is clear from this study that the Comintern was a predominant factor in keeping prewar Communism alive in Japan, with so many Japanese going to the Soviet Union and many being trained at the Eastern Workers Communist University, *Kutobe*, one wishes that space might have been found to collect in one chapter the detailed information on that subject alone. Because the book contains so many details, the reader will find himself returning to it often.

Columbia University

HUGH BORTON

SAIONJI-HARADA MEMOIRS: FRAGILE VICTORY. PRINCE SAIONJI AND THE 1930 LONDON TREATY ISSUE, FROM THE MEMOIRS OF BARON HARADA KUMAO. Translated with an introduction and annotations by *Thomas Francis Mayer-Oakes*. (Detroit: Wayne State University Press. 1968. Pp. 330. \$10.00.)

THE *Saionji-Harada Memoirs*, first made broadly known through an English translation used in the Tokyo war crimes trials, significantly elucidates the behind-the-scenes world of prewar Japan's decision making. In the latest version Professor Mayer-Oakes focuses only upon that section devoted to the issue of the London Naval Treaty. His translation is based on the first of the nine-volume Iwanami publication of Baron Harada Kumao's diary entitled *Saionji kō to seikyōku* [Prince Saionji and the Political Situation]. Harada's diary is "a meticulously detailed report of the political intelligence he gathered for Saionji," that sole surviving Genro and the last Japanese deferred to as "uniquely representing the Emperor's will."

Mayer-Oakes enhances the value of Harada's diary by his introduction and numerous explanatory and bibliographic notes. He points out ambiguities and clarifies the circumstances associated with peculiarities of Japanese political behavior, with Harada's task of communicating with individuals of various station, and with unstated passages taken for granted by those for whom the diary was meant to be read. Especially instructive is his discussion of the theory and practice of the imperial institution, the Genro, and the Supreme Command. However Mayer-Oakes's succinctness might well have given way to expansive explanations of the fascinating and really basic question dealing with the responsibility of advisers in a state based on the doctrine of transcendent imperial sovereignty. Indeed, the whole issue of the London Naval Treaty pivoted on the opposing responses to the questions, "Where does ultimate authority for decisions rest?" and "Who is the final adviser?"

In this vibrant and accurate translation Mayer-Oakes offers the English reader a prism to observe how Saionji assisted the Hamaguchi government to maneuver the treaty past the formidable opposition of the chief of the naval general staff, Katō Kanji; the Supreme War Council chaired by the revered Fleet Admiral Tōgō Heihachirō; and the Privy Council conservatives led by Hiranuma Kiichirō and Itō Miyoji. Throughout the episode, the Minister of War, General Ugaki Kazunari, showed only token interest in "the Navy's problem." Despite Ugaki's illness during much of the time, it is difficult to avoid the notion that the diary portrays army authorities as lack-

ing in prescience. But that portrayal is not the work of the author, who wisely cautions that "Like Stendhal's observer at Waterloo Harada is eyewitness to large events . . . yet his view is from a position of limited horizons."

University of Oregon

G. RALPH FALCONERI

KOREA: DEMOCRACY ON TRIAL. By *John Kie-chiang Oh*. (Ithaca, N.Y.: Cornell University Press. 1968. Pp. xiv, 240. \$7.95.)

WITHOUT Professor Oh's careful and objective study, the long background of a political crisis in Seoul during the fall of 1969 might not be so thoroughly understood. Once again (as in the case of President Syngman Rhee), the personality of a single leader, General Chung Hee Park (Pak Chŏng-hi), seemed to raise the specter of "personalism," as opposed to "institutionalism," and to place Korean "democracy on trial." As expected, the attempt to set aside a constitutional limitation of two successive terms as President sparked bitter and violent response on the part of fragmented opposition parties and of students.

As is made clear in this study, the Republic of (South) Korea, like other emerging nations, has actually faced numerous political problems: the legacy of harsh colonial rule by Japan; division of the country; war that ravaged the whole peninsula; popular uprisings that toppled the seemingly impregnable Rhee regime; a military *coup*, rule by decree, and entrenchment of the Third Republic. All of these factors are ably managed by the author, who has done extensive research in original and English sources, supplemented by Japanese materials. The result is the most authoritative study in English of South Korean politics.

In light of contemporary events, the most interesting analyses are of two selected opposition forces. Divergent views of student dissent are covered, ranging from "political development" through "democracy" to "revolution." Equally impressive is Oh's characterization of the military, which, as a result of the Korean War, became the fourth largest armed force in the world. In postwar Korea, "the armed forces remained the most powerful and effective organization in the nation, in both the absolute and the relative senses." In contrast, other institutions have been "embryonic, parasitic, or anemic."

Fascinating indeed, and now timely, is the brief section on "General Park: The Man and His Ideas." Obviously, according to the author, the man has concentrated less on "belief in democracy," and more on the projection of "an image of a heroic leader." On the other hand, Park has profited from the spectacular success of the First Five-Year Plan (1962-1966), which produced an average annual gain of 8.5 per cent in GNP, far higher than that of any Asian state. A longer time, Oh thinks, will prove whether economic progress has been matched by political development and sophistication on the part of citizens of the Republic of Korea.

The author was born in Korea and studied first at Seoul National and later at Marquette and Columbia Universities. With a doctorate in international relations from Georgetown, he served on the Korean Mission to the UN in 1957-1958. At present he is associate professor of political science at Marquette University.

Rutgers University, New Brunswick, New Jersey

ARDATH W. BURKS

THE INDIAN PRESS (A COLLECTION OF PAPERS PRESENTED AT THE FOURTH ANNUAL CONFERENCE OF THE INSTITUTE, MYSORE—1966). Edited by S. P. Sen. (Calcutta: Institute of Historical Studies. 1967. Pp. 166. \$6.00.)

IN the study of modern Indian history the development of the press is a subject of particular importance. This is because in India, unlike many Western nations, the press developed well in advance of either representative political institutions or popular education. During the nineteenth century the Indian press, in many respects, filled the role of an opposition political party while its function as an educator and agent of social change was more significant than in many other countries. Yet, rather surprisingly, apart from two or three general studies, comparatively little work has been done on the development of the Indian press. The Institute of Historical Studies, Calcutta, deserves to be complimented on devoting an annual conference to this subject and, in particular, for focusing attention on the vernacular press, the "grass roots" of the press in India.

Unfortunately, however, the caliber of the articles rarely measures up to the potential of the subject, and the volume as a whole, as Dr. Sen acknowledges in his preface, suffers from wide disparities in content and subject matter. While the emphasis of the volume is on modern India, two articles are included on news writing in the medieval period. The latter subject is interesting; in fact, Dr. J. N. Sarkar's article on "News-Writers of Mughal India" is undoubtedly the most scholarly in the whole volume, but the connection between it and modern journalism is remote. Of the other articles, there is too much overlapping between the two on Bengali journalism; the same is true, to a lesser extent, of the two brief papers on the Kannada press. But the main weakness of these four papers, as well as of the remaining ones on the Hindi, Urdu, Tamil, and Telugu press, is that they tend to be more descriptive and chronological rather than analytical and thematic in their treatment. All of them provide much useful factual information regarding the various publications, and scholars will also find the comprehensive bibliography helpful. Perhaps the greatest service of the volume, however, will be to stimulate further scholarly inquiry into the growth and influence of the vernacular press in India.

University of Manitoba

E. C. MOULTON

GANDHI AND MODERN INDIA. By *Penderel Moon*. (New York: W. W. Norton and Company. 1969. Pp. 312. \$6.95.)

MOST biographies of Gandhi have been written by devotees and admirers, with the result that they are commonly quite uncritical in their approach to Gandhi and his place in India's modern history. Penderel Moon's new book falls into a different category. He writes from the perspective of an Englishman who spent fifteen years in the Indian Civil Service (1929 to 1944), and his book is refreshingly free from the usual pieties.

His central concern is with Gandhi's role in the political leadership of Indian nationalism, and he gives relatively little attention to other dimensions of Gandhi's career as religious teacher and as social reformer. As a result, the core of the book consists of a detailed account of the interplay between the Indian National Congress and the British authorities; the Muslim League and other Indian political groups periodically enter into the narrative. Unfortunately, this sometimes makes for rather arid

reading when the subtleties and technicalities of intricate negotiations are being traced.

Moon's judgments, often shrewd and pointed, on Gandhi's methods and actions are worthy of careful consideration. Yet there is a dangerous blind spot in Moon's approach: his credulous acceptance of the mythology that British rule was intentionally aimed toward India's freedom. Thus, he writes that "the civil disobedience movement [of 1930-1931] did not deflect the British from their purpose. Their policy of advancing India stage by stage along the road to full self-government remained unaltered."

Such simplistic nonsense is out of place in a work that is presented as a serious historical study. Moon has the perspicacity to see through Gandhian poses. Yet he fails to achieve comparable insights with respect to British policy and the diverse motivations from which it was derived. Moon specifically rejects the notion that Gandhi was "the author of India's freedom," but he does so on the implausible ground that India had already been clearly launched on the path to independence before Gandhi's entrance into nationalist politics in 1919.

Given this set of underlying assumptions, it is not surprising that Moon's book does little to advance our understanding of the complex interaction between Britain and Indian nationalism in the interwar years. Its main value lies in the fact that it provides a useful antidote to the more mindless enthusiasms of Gandhilotry.

Sir George Williams University

MARTIN DEMING LEWIS

THE BIRTH OF INDIAN CIVILIZATION: INDIA AND PAKISTAN BEFORE 500 B.C. By *Bridget and Raymond Allchin*. [Pelican Books.] (Baltimore: Penguin Books. 1968. Pp. 365. \$2.25.)

THE need for a general account of the prehistory of South Asia has grown dramatically since Pelican published Piggott's *Prehistoric India* in 1950. Two related developments have created this need and at the same time made it possible to fill it. The great number of excavations that have been carried out in the last twenty years have greatly augmented the previously meager body of data available on prehistoric India and Pakistan. At the same time, the proliferation of these reports has also increased the number of problems that need solving. For example, we now know that the so-called Indus Valley civilization extended as far as the Ganges in the northeast and as far as Gujarat in the southeast; that there were numerous Neolithic and Chalcolithic cultures all over India, especially in the Deccan and the south; that a "Black-and-Red Ware" culture flourished in an area forming an arc from Bengal and Bihar in the east to Rajasthan in the west; and that an "Iron Age Painted Grey Ware" culture flourished in the Ganges Valley. Before 1950 the chronological relationships of excavated cultures in different regions were difficult if not impossible to establish. Since then, the availability of a large number of radiocarbon dates has made it possible to establish an absolute chronology for these and other cultures in the different regions of the subcontinent. The Allchins have done an admirable job of creating order out of this mounting chaos.

Part One of the book begins with a chapter on geography and continues with a comprehensive, detailed, and integrated chronological, region-by-region account, beginning with the early Stone Age and ending with a discussion of the Gangetic (Aryan) civilization before 500 B.C. Part Two contains chapters on settlement patterns, agriculture, trade, and technology. A small chapter deals with art and religion. The

book also includes thirty-two plates of photographs, seventy-five instructive maps and diagrams, a select list of radiocarbon dates, a select bibliography, and detailed references for each chapter.

The authors have adopted what they call a "geographical approach" in their book; that is, they have proceeded to isolate clusters of cultural traits by region and to analyze their relationships with each other. The authors succeed in using this approach. They avoid the "nose to the ground" reluctance of some archaeologists of India to offer interpretations of their data, while at the same time avoiding the heady speculations engaged in by others. Some of their arguments will, I am sure, cause some anxiety and distress. To give only two examples, Aryan invasions, floods, and soil exhaustion have all in their turn been used to explain the decline of the Indus Valley civilization around 1750 B.C. The authors take the position, which they support with new evidence, that the decline was certainly accompanied, if not caused, by intrusions of "Aryan" foreigners from Iran and Central Asia. The second example is the authors' attempt to relate the "Black-and-Red Ware" culture to the complex of cultures and events of the post-Indus period. Here the Allchins revive a once-popular view that there were two major "waves" of Aryan intrusions and settlements. They argue that the second wave gradually pushed the first Aryan settlers out of the upper Ganges Valley. The second wave of settlers is linked to the "Painted Grey Ware" culture and the Aryans of the Vedic texts (ca. 1000-500 B.C.), while those displaced are linked to the "Black-and-Red Ware" culture and the ancestors of the later speakers of Bengali and Marathi. I find these and other arguments of the Allchins' convincing for the most part. Their book is a major contribution.

University of Chicago

RONALD INDEN

BREAKTHROUGH IN BURMA: MEMOIRS OF A REVOLUTION, 1939-1946.

By *Ba Maw*. (New Haven, Conn.: Yale University Press. 1968. Pp. xxiii, 460. \$8.75.)

Dr. Ba Maw was one of the colorful personalities of the nationalist movement of pre-war Burma. An intelligent, adaptable person, he was also highly emotional, strongly Anglophobe, politically ambitious, and personally vain. He was a bit too Europeanized to identify with the Burmese people generally, whom he regarded with some measure of condescension. He lost out politically during the war as a result of his too thorough-going acceptance of Tokyo's Greater East Asian objectives, in which he developed a personal stake. Ba Maw's manuscript account of his pre-1945 role is one of the few historically significant items to be smuggled out of Burma during the blight of the Ne Win dictatorship, and its publication is most welcome. What is lacking in the two forewords, particularly, is some acknowledgment that the book is a highly ex parte account, and that it displays many of the values and shortcomings characteristic of memoir presentations. Perhaps the most valuable portions of the story relate to the dilemma posed by the outbreak of the European war in 1939, to Ba Maw's early contacts with Japanese agents, including the amazing Colonel Suzuki, and especially to his moving account of the British defeat and the tragic exodus of the Indian population. As an important actor in Burma's pre-1945 history, Ba Maw deserves to have his say.

Several examples of his deficiencies as a historian will have to suffice. Ba Maw omits or distorts historical evidence that fits badly into his assumed posture as the nationalist hero of Burma's break-through. Few informed outsiders and no Burmans

will be impressed by his claims. The Saya San rebellion, which brought Ba Maw into the political limelight, was not a mass peasant rising, and reference to the 1936 student strike is omitted apparently because Ba Maw, as Minister of Education, was embarrassed at the time. He characterizes the Thakins generally as the products of a social eruption of depression times, which brought to the surface the economically unemployed and rootless, a few good ones mingled with a vast crowd of the worst. Their very deficiencies made them qualify as the authentic representatives of the frustrated masses. Ba Maw's Sinyetha party did not, as claimed, win the election of 1936, and he became Premier only with the support of dissident and minority representatives in the Council. He makes only one apology for his pro-Japanese stand, namely his role in recruiting some 65,000 forced labor victims for use on the Burma portion of the "Railway of Death." There is doubtless some need to reassess Aung San's historic role, but Ba Maw's case is not strengthened by his spiteful characterization of Burma's national hero as a moody, befuddled, insecure young man, twice a turncoat, and the author of the naked Japanese-style military oppression of General Ne Win. Aung San will not lack defenders.

Ohio University

JOHN F. CADY

THE UNITED STATES AND MALAYSIA. By *James W. Gould*. [The American Foreign Policy Library.] (Cambridge, Mass.: Harvard University Press. 1969. Pp. x, 267. \$6.95.)

PROFESSOR Gould of Scripps College and Claremont Graduate School, and a former Peace Corps director in Malaysia, has written the latest addition to "The American Foreign Policy Library" series, and his book has the virtues and the many drawbacks of its companion volumes. The general reader wanting a rough, usually a very rough, over-all impression of Malaysia's history, politics, and current problems can get it in these pages. There are brief chapters or sections on geography and population, government and social development, economics, educational and language problems, and relations with People's China, the Philippines, and other Asian nations, as well as with the US. Few facets of Malaysia's major domestic predicaments, from the Chinese-Malayan communal problem to Kuala Lumpur's frequently strained relations with the Borneo states, do not get at least some mention here, if only in passing. With some exceptions Gould's grasp of Malaysia's foreign policy interests is generally adequate, and it is useful to have his concise summary, however brief, of American concerns with the region. But, probably not least because of the demand of the format of the series, Gould's volume is so studded with inaccuracies, faulty (because unqualified) or misleading generalizations, and serious omissions, that some may well question its value even for the beginning reader.

In the chapter on government and politics there is, for example, no mention of the Democratic Action party nor of the *Geraġan Ra'ayat Malaysia*, while the complex reasons for the dissatisfaction and aspirations of the Chinese democratic Left and its allies in the Malayan intelligentsia are virtually ignored, although all of these form the essential background to the elections of May 1969 and their tragic aftermath. Gould does not distinguish on pages 110-11 between the Malayan Communist party and the Clandestine Communist Organization in Sarawak, and it is simply incorrect to say that after the end of the Indonesian Confrontation in 1966 "most of the guerillas were rounded up." To the contrary, most of the guerrillas found new organizational havens in the Pasokan Gerilja Rakjat Sarawak and later the *Paraġu*, active since the close of

1967, but with which the author appears to be unfamiliar. Gould's analysis of the origins of the anti-Malaysian Indonesian confrontation campaign is particularly defective because he appears to be unaware of the link between this campaign and Indonesia's New Guinea policy and its domestic implications, particularly the role of the Indonesian Communist party. Hence, the curious circumstance wherein Indonesia first officially welcomed Malaysia in November 1961 before turning against it is not considered either.

The author also appears unfamiliar both with Rahman's interview in *The Straits Times* of November 20, 1968, on the subject of Communist China's foreign policies and with the Malaysian government's White Paper, *The Path of Violence to Absolute Power* (1968), or he would not have written that "in recent years there has been a mellowing of Malaysia's views on China"; the reverse, rather, is true. Islam is described as re-enforcing the Malay attitude of fatalism, a view that does not explain why, elsewhere in Southeast Asia, Islamic influence has had such an energizing effect. Nor is it factually correct to say that the Dutch, after they conquered Malacca in 1641, "proceeded to neglect the port in favor of Batavia." Malacca remained vital to the Dutch East India Company's Asian entrepôt trade. The author's view of the Bugis fulfilling a role similar to "Swiss mercenaries" hardly explains their enduring presence in Malaya, especially not since the author later notes that "the Bugis sent colonists to Selangor." And does the author really believe that the Malay has greater need of "privacy" than the Chinese?

University of Bridgeport

JUSTUS M. VAN DER KROEF

ROYAL AUSTRALIAN NAVY, 1942-1945. By G. Hermon Gill. [Australia in the War of 1939-1945, Series 2 (Navy), Volume II.] (Canberra: Australian War Memorial; distrib. by Angus and Robertson, Sydney. 1968. Pp. xviii, 753. \$A4.00.)

THE Royal Australian Navy in World War II consisted of a small force of cruisers, destroyers, and lesser vessels operating almost invariably as parts of great Allied fleets and task forces commanded by American and British admirals. In the period covered by this volume, the second in the official history of the Australian Navy in World War II, Australian naval units served principally in the areas of greatest concern to the home country: in General Douglas MacArthur's Southwest Pacific Area as part of the US Seventh Fleet and with British Empire fleet units in the Indian Ocean. The threat to Australia posed by the Japanese thrust southward following Pearl Harbor forced the withdrawal of all but minor units from the British fleets operating in the Atlantic and Mediterranean.

Telling the story of naval units scattered among the fleets of greater allies is a peculiarly difficult task. G. Hermon Gill has attempted to do so by fitting it into the larger history of naval warfare in World War II, concentrating particularly on the Pacific and Indian Oceans. This represents a tour de force that he is not always able to bring off successfully. Transitions from one sphere of the naval war to another are often abrupt, and it is difficult at times to grasp the role of the Australian Navy amidst the sound and fury of the major actions of the US Pacific Fleet. That role does, nevertheless, eventually emerge, and the details on the operations of Australian ships in such battles as the Coral Sea and Leyte Gulf represent an original contribution. Since much of the book is concerned with the story of major naval actions of the war, this contribution is limited, for the author adds little to the works of Samuel Eliot Morison and Captain S. W. Roskill covering the American and British

naval actions of the war. Indeed, Gill relies heavily on and quotes at length from Morison and Roskill, using their works as sources on strategic decisions and land actions far better covered in official British and American military histories. He does disagree at times, in the interests of Australia, with Morison's conclusions and interpretations, and these disagreements are provocative.

Overall, Gill shows a single-minded devotion to the Mahan thesis of the importance of sea power and attributes the Allied victory to superiority at sea without much consideration of the relative contribution of land and air forces. The book is sparsely documented and contains no bibliography though it is evident that the author has used Australian naval records to good advantage. The volume, along with its predecessor, will serve as a valuable reminder to Americans of the importance of the naval contribution of a smaller ally to victory in the Pacific in World War II.

Department of the Army

ROBERT W. COAKLEY

Americas

A HISTORY OF PUBLIC HEALTH IN NEW YORK CITY, 1625-1866. By *John Duffy*. (New York: Russell Sage Foundation. 1968. Pp. xix, 619. \$11.50.)

THE enactment of the New York Metropolitan Health Bill in 1866 marked a decisive turning point in the history of public health not only in New York City but in the United States as a whole. A basic problem that concerned pioneer public health workers was the lack of adequate administrative machinery. The civil service during the early nineteenth century was small in numbers, limited in function, and recruited almost wholly by patronage. Change from a haphazard to a more efficient administration was essential to the development of an increasingly complicated urban society. In fact, the provision of a stable administrative structure made it easier to incorporate new knowledge into public health practice. It was in New York City that such an organization was first created in the United States, as a model for others to follow.

In 1966 the New York City Department of Health celebrated the centennial of its forerunner, the Metropolitan Board of Health. As one of the activities marking this anniversary, the department invited Professor John Duffy to write a history of public health in New York City. The first of the projected two volumes has now appeared, covering the evolution of community action with respect to health from the foundation of the town by the Dutch to the culmination of the sanitary reform movement in the establishment of the Metropolitan Board of Health. Based on extensive use of manuscript and printed sources, as well as of the relevant secondary literature, Duffy's study makes a major contribution to the history of public health in the United States. Beginning with the Dutch frontier post established at the mouth of the Hudson, the author traces the growth of the community from village to town to city and the means by which, at various stages, efforts were made to deal with problems of public health. Duffy's far-reaching account includes within its scope epidemic diseases, street sanitation and nuisances, water supply and sewerage, food and market regulation, hospitals and medical care, social welfare, public administration, and immigration. The result is as much a social history of New York City as an account of its health aspects.

While the general outlines of the history of public health in New York City are not new, earlier versions have been supplemented with considerable detail. Moreover,

whenever the national history of public health in the United States is written, it will have to be based on a number of local and state histories. Duffy has produced two useful examples in his earlier history of medicine in Louisiana and in the present history of public health in New York City.

Finally, it is important to keep in mind that a complex urban civilization requires appropriate societal mechanisms for handling a variety of group problems, not least those concerned with health. Since we are today in a period of change analogous in some ways to that of one hundred years ago, Duffy's perspective is a useful one.

The book is well produced, convenient to use, and relatively reasonable in price. It certainly deserves to be read and used by American social historians, historians of medicine and public health, those in the health professions, and others concerned with urban health problems.

Yale University

GEORGE ROSEN

PURITANISM AND THE WILDERNESS: THE INTELLECTUAL SIGNIFICANCE OF THE NEW ENGLAND FRONTIER, 1629-1700. By *Peter N. Carroll*. (New York: Columbia University Press. 1969. Pp. xi, 243. \$7.50.)

THE avowed intent of this volume is the determination of "the role of the wilderness in the thought of . . . the settlers of New England." In pursuing this end, the author has ransacked the writings of New England's "intellectual elites" and strung his findings together in a narrative bristling with quotation marks. The result is a sequential series of definitions of "wilderness" as reflected in New England writings to 1700, all of which seem to add up to this: wilderness as Biblical metaphor was imported as part of the cultural baggage of the New Englanders to describe both the reality of the New World and the social mission of the Puritans; yet ultimately the real wilderness was not only discerned apart from the metaphor, but worked against both metaphor and social mission.

That a reviewer so tentatively puts forth the thesis of a book can only be deplored. But the fact is that the volume is more confused than cogent. It is frustratingly repetitive. We are, for example, offered three slightly variant versions of the line, "the ocean voyage preconditioned the Puritans for the novelties of the American wilderness" within three pages. The author uses "Puritan," "New Englander," and "colonists" synonymously and regularly displays the difficulties of proceeding without a definition by almost apologetic phrases to the effect that a particular "Puritan" idea, "to be sure," was "shared by most Englishmen." Above all, while he recognizes the problems of intellectual history and acknowledges in his preface the uncertain relationship between ideas and reality and between ideas as articulated by the few and held by the many, these problems are ignored in the text. We are offered such simplistic assertions as "George Cleeve of Casco Bay implied the true sentiments among the mass of people"—so much for the relationship of the intellectual elite and the lower sort of men; and "the commitment to community in Puritan Massachusetts represented more than a theoretical exercise; it also satisfied the needs of a frontier society"—so much for the relationship of ideas and reality.

That the American wilderness as it was and as it was discerned had an effect upon Puritanism and upon men in general is certain. Many have written around and on the subject. Psychology, sociology, anthropology are even now offering us tools by which to bridge the gap between the articulate who, in the main, leave us the evidence of the past, and the inarticulate who are history's proper subject. With time

and thought, Carroll might well have contributed vastly toward the understanding of a very complicated question, and his plethora of data, the very awkwardness of his Puritanism, and the prefatory remarks as to the difficulties of intellectual history indicate as much. That he has not seems the regrettable consequence of haste.

University of New Hampshire

DARRETT B. RUTMAN

THE VIRGINIA DYNASTIES: THE EMERGENCE OF "KING" CARTER AND THE GOLDEN AGE. By *Clifford Dowdey*. (Boston: Little, Brown and Company. 1969. Pp. x, 438. \$10.00.)

This is the kind of book that drives a professional historian reviewing it almost to distraction. It calls to mind the work of a genius who flaunts rules of the craft—perhaps because he skipped the apprenticeship—yet says something significant in an unmistakable way. On one side, the scales are weighted with factual errors, occasional anachronisms, and questionable judgments on subordinate matters. On the opposite arm, the burden is a fundamental insight, presented with conviction and art. Who would not, however uneasy in professional conscience, forgive venial sins for the greater good?

The Virginia Dynasties deals with the fifty years between Bacon's Rebellion and the death of "King" Carter in 1732. Immediately preceding Virginia's "golden age," this half century is a kind of formative period in which the author finds two parallel processes resolving themselves. In politics the growth of a governing caste—"country born" and capable—gave Virginia a characteristic leadership and behavior. At the same time a merchant-planter class emerged to organize and superintend the economy of the colony. Though the two processes were intimately related, the latter has moved in eclipse behind the seductive caricature of the planter as socialite and statesman. The author's emphasis on the importance of the merchant-planters and his analysis of those prosaic enterprises yielding fortunes that differed from those of ordinary tobacco producers are the outstanding contributions of this book.

Students of the eighteenth-century Chesapeake can hardly be surprised at Mr. Dowdey's reading. From Bruce and Bassett down to the present, the secondary literature has contained evidence that should long ago have been construed to clarify the economic rise of the "gentry" or "aristocracy." More recently an article here and there has sketched the architecture of an individual fortune or has spoken of fortune building in general. But, prior to *The Virginia Dynasties*, no one has so clearly described these entrepreneurs (the merchant-planters) in their multiple pursuits as merchants, land speculators, factors, and exchange bankers and brought them properly into the mainstream of Chesapeake history. Consequently Freshmen still recite the ancient half-truths about planters, and even graduate students carve up Virginia into great tracts cultivated by slaves while the masters dispensed lavish hospitality and slipped mindlessly into debt. What the intelligent layman believes, if the subject crosses his mind at all, might be interesting to hear. Now, at last, Dowdey puts the economically successful Virginian in his proper historical setting and gives him a functional label: merchant-planter. At least one scholar on the brink of publication is left with the feeling that he has been scooped.

The Virginia Dynasties is not documented. It is not clear whether the author arrived at his results from new research in the sources or from perceptive reading of the bulky and sometimes undependable secondary literature. Quite likely both, with each reinforcing the other, to judge from his select bibliography. But the ques-

tion, as with all inquiries about the origin of an important insight, does not really matter. The results count: the emphasis on the importance of the merchant-planters and the analysis of the enterprises that give them a surprising resemblance to managers of lucrative agrocommercial complexes today.

In elaborating his theme the author makes choices, as historians must when they realize that they cannot accomplish everything. Instead of the broad context of the imperial school, which emphasized relationships among elements of the Empire frequently from the vantage point of Whitehall and the London merchant community, he focuses on provincial Virginia; this is also a necessary component for understanding the resolution of tensions and accords within the English-speaking world. He keeps the commonalty well in the background, because this is a study in power, economic and political, and therefore of personages. Even within these limits Dowdey claims no more than a narrative presentation that stems from his early writing as a novelist. Accordingly, let us not talk about the final word. There is still much to do, but it will be done in air that has been cleared. And let it be said that again a nonprofessional has scored. If the sequel promised by his publishers does as much for the "golden age," some among us may wonder whether bohemia does not offer better preparation for historical writing than the graduate thicket in the grove of academe.

University of Georgia

AUBREY C. LAND

THE FOUNDING OF A NATION: A HISTORY OF THE AMERICAN REVOLUTION 1763-1776. By *Merrill Jensen*. (New York: Oxford University Press. 1968. Pp. xiii, 735. \$13.50.)

A HISTORY OF THE AMERICAN REVOLUTION. By *John R. Alden*. (New York: Alfred A. Knopf. 1969. Pp. vii, 541, xvi. \$10.00.)

THE publication of big books on the American Revolution by two senior scholars in the field is an occasion for special notice. The bigger book, by Professor Merrill Jensen, deals with events down to the Declaration of Independence, and, in effect, completes the trilogy—first volume last—begun three decades ago with *The Articles of Confederation*, and carried forward in *The New Nation*. Professor John Alden has surveyed the whole Revolution in a single volume, a task for which his earlier books in the "New American Nation" and "History of the South" series have prepared him. On the surface the two books under review appear similar: they are both "straight" histories; that is, they are narrative accounts emphasizing politics. This traditional approach to a subject rich in general narrative as well as monographic work naturally raises questions: Are the new books better? Do they add to knowledge or improve understanding? Each author has addressed himself to these questions. Alden says in the first sentence that his "version of that momentous convulsion differs from all those offered by other historians," and goes on to point out that, surprisingly, no "modern and relatively detailed book devoted precisely to the era of the Revolution" exists. Jensen is more modest; refusing to join the search for causes, motives, or the deeper meaning of revolutionary rhetoric, he aims "to set forth as fully as possible the complex history" of Anglo-American political conflict from 1763 to 1776. With the authors' rather different intentions in mind, comparison reveals, beneath the surface of a familiar story being told once again, two very different kinds of historical writing.

Without question, Alden was engaged in writing a popular history, directed be-

yond the academic community. There are no notes, although a long, somewhat perfunctory "Essay upon Authorities" signals that the author is a serious scholar. The narrative moves briskly, pausing only for an anecdote, a biographical sketch, or brief reflections on ultimate consequences. The style and tone are evidently meant to convey a hardheaded understanding of men along with a proper respect for the magnificent drama in which they played parts.

Jensen, on the other hand, had scholars rather than general readers in mind. He writes clearly and forcefully but without ornament; he cites evidence without embarrassment. He has put his account together bit by bit, and, if parts of the painstakingly assembled narrative detract from the dramatic quality of the whole, he does not seem to have been troubled. He is concerned less with the biography of leaders than with their public actions, especially with establishing exactly what was said or written, and the exact sequence in which events occurred.

The differences in form and intention between the books reflect differences on basic issues: for example, "Newspapers are especially bad guides to sentiment, for they dote upon extremes and feed on fighting . . ." (Alden, p. 125). In contrast, "Newspapers played a tremendous role in political life from the Stamp Act onward. . . . The newspapers, far more than most pamphlets, appealed to the mass of the people, stirred their emotions, and urged them to action" (Jensen, pp. 127-28). When the authors deal with the same events, these differences affect interpretation, however much each may have sought to avoid any tiresome commitment to a frame of reference. A few more examples make the point. Jensen gives forty-eight pages to the second nonimportation movement, 1767-1770, paying close attention to the sources of pressure behind the movement and to the problems it encountered in trying to create a united American front against the Townshend Acts. The general reader may find these pages heavy going. Alden handles the same events, not in the ten or fifteen pages that would have been proportionately equivalent to Jensen's longer work, but in a few sentences: "Gradually, during the years 1768 and 1769, the colonists established committees from the district of Maine to Georgia that enforced the boycott. So-called 'non-importation agreements' became the orders of the day, and they were zealously executed by forthright defenders of American liberty. Some recalcitrant merchants, threatened by ostracism and even with physical violence, had to yield to the general will. Imports of British goods sank remarkably." The difference here is more than one of length or taste.

The Alden version does not transmit any sense of the deep suspicion and residual bitterness between colonies that nearly killed the movement and helped to poison continental politics, nor of the desperate attempts by nonmercantile groups to keep the movement alive, nor of the consequent alarm and loss of control by the merchants who had been able to manage the first nonimportation movement. Esmond Wright, in his much briefer *Fabric of Freedom, 1763-1800*, caught these essential features in a page or two, but Alden may have submerged the details because they might have obscured the theme of American anger and unity growing in response to British provocation.

Similarly, in their accounts of the calling of the First Continental Congress, and of the Declaration of Independence, Jensen reconstructs the complex of events in a way that describes more fully than anything else I have read the character and magnitude of resistance to calling a congress in 1774 and declaring independence in 1776, while Alden is content to evoke the mood that led toward what, it seems, only the confused, the frightened, and the intransigent did not know was coming. Even

on the West, where Alden, as might be expected from his previous work, writes at relatively greater length, the difference in treatment is significant. Alden presents Dunmore's War as part of sporadic Indian resistance to the relentless push of white settlers; when the Shawnee fought back in 1774, the governor of Virginia raised the militia "with almost astonishing celerity" and destroyed their villages. But Jensen describes the war as part of the continuing crisis in the Virginia economy, which nourished extravagant hopes (among the militia and others) for western lands, of the unsettling effect on Virginia politics of the rambunctious Governor Dunmore, and of the violent struggle between Virginia and Pennsylvania for control of the upper Ohio Valley. Jensen makes the war relevant to the Revolution, while Alden suggests only that, meanwhile, out West, restless Americans were ever on the move.

Once into the Revolution itself, Alden has concentrated heavily on military events. He knows these campaigns very well, and his handling of strategic decisions and the movement of forces on both sides is sure. The same cannot be said of his brief account of "The War at Home." We are told that there were conservatives, radicals, and liberals who were "torn by jealousies and divisions among themselves," but that there was "no great domestic upheaval," no real revolution in the French or Russian sense, and thus no problem in achieving and maintaining political stability. The argument and even its categories fail to satisfy, not because they seem mistaken but because they are vague and ambiguous. The final chapter, really an epilogue, which carries the story from the end of the war to 1789, presents what can be called a Whig interpretation; less obviously, such an interpretation pervades the whole book. Throughout, questions of explanation appear simpler and less urgent to the author than they do to me. Surely some concessions have to be made to the general reader, but these, I think, are not the ones to make. There is little in the book to support its claim of freshness and originality.

Jensen's book is also open to criticism, but on different grounds. He has not forsworn the class analysis of politics, but neither has he argued vigorously for it as he did in the two earlier volumes of his history of the Revolution; rather, class analysis is allowed to slip quietly into the discussion from time to time. But his approach, mainly through political literature and correspondence, makes it difficult to support or test the idea that the politics of the American Revolution had this, or any other, social configuration. Some readers will be distressed that the book is so little concerned with the currently most active lines of inquiry in the field, particularly the reconsideration of rhetoric and the quantitative study of society. Yet the author, working within self-imposed limits, has reminded us that there is more to be known about revolution than ethos and social structure, that revolutions have most directly to do with political decisions, organizations, and power. Within those limits, Jensen has written a valuable book, one that does not offer any bold new interpretation, but scrupulously re-creates the texture of revolutionary politics. It will not change minds so much as it will strengthen the understanding of those who read it and refer to it.

University of Michigan, Ann Arbor

JOHN SHY

AMERICA AND THE MEDITERRANEAN WORLD, 1776-1882. By *James A. Field, Jr.* (Princeton, N. J.: Princeton University Press. 1969. Pp. xv, 485. \$13.50.)

In this documented study, based on extensive use of the archives of the State and Navy Departments, of Protestant missionary organizations, and of various printed

and secondary sources, Professor Field of Swarthmore College draws attention to an area usually neglected in diplomatic histories: America's relations with the lands bordering the Mediterranean Sea. His concern does not, as the book's title suggests, encompass the entire Mediterranean world. It includes much more discussion of developments in the countries of the eastern Mediterranean and the Near East than of those in Spain, France, Italy, and the European countries of the western end of the closed sea; more attention is given to American naval contacts, maritime trade, and missionary activity than to traditional foreign relations.

Since early diplomatic relations with that part of the world were not especially important to the United States, Field's method of investigation is logical. He wisely goes beyond official contacts to explore various activities, private and official, of the few Americans scattered through the area during most of the nineteenth century, or until the British occupation of Egypt in 1882. He sees those activities as falling into four main categories: the effort of Americans to build trade, their use of the navy as an instrument of policy, their Protestant missionary campaigns, and their "secularized missionary spirit" based on the idea of freedom and "self-determination of peoples." This four-dimensional approach, which he suggests is new in the study of American foreign relations, assumes that private individuals were as important in the shaping of foreign policy toward the Mediterranean world as were government officials. This approach, according to Field, also reflects two aspirations in American foreign policy: one practical, the other utopian. Pragmatically, Americans sought trade and profits; idealistically, they wished for the perfection of man and of society.

This latter, utopian facet of American attitudes toward the foreign world, Field believes, produced a notable missionary and educational effort in the Near East and stimulated private and public efforts to contribute to economic development there. He sees this sense of uplifting humanitarianism as an aspect of what he calls the American enlightenment, something evident in the work and ideals of the traders, Protestant missionaries, naval men, and diplomats in the Mediterranean. Their belief in the rational as a means for solving human difficulties, their hopes for the millennium, and the other ideals of the American enlightenment, he concludes, still form the doctrine for America's role in the world.

While laudable, this effort to find unity, purpose, and enduring ideals in American policy in the Mediterranean does not, in my judgment, succeed. It fails because the intellectual framework cannot provide a unity that does not exist in American policy; it fails also because the author's treatment of his subject and his perspective are narrow. Writing primarily from American- and English-language sources, and from what he calls the American viewpoint, he has produced a one-dimensional, episodic history, one that lacks real continuity and depth and that is nationalistically and religiously partisan. It shows no real appreciation for the feelings and cultural attitudes of the peoples who were the objects of American policy.

The author exaggerates the importance of the missionary movement, writing, for example, that "the decision of these early-nineteenth-century Americans to undertake the evangelization of the world must seem a little breathtaking." To assume that American missionaries, whose evangelizing efforts were resented and never succeeded, were engaged in a civilizing and uplifting mission to peoples who had ancient and proud cultural and religious traditions of their own seems self-centered and insensitive rather than breathtaking. This book even shows an old-fashioned anti-Catholic bias, coming perhaps from the author's immersion in the partisan literature of the Protestant evangelizers. For instance, he refers to Catholic mission-

aries in the Near East as "the sentinels of the Mother of Harlots" without use of quotation marks.

Despite its flaws, this effort to go beyond official diplomacy, to probe religious, naval, and commercial activities, and to evaluate their impact on American relations with other peoples is commendable. But it does not, in my view, go far enough. The book would have greater depth, even though written with emphasis on "American actions and American aims," if it showed some understanding of other cultures, some insight into the feelings of other peoples. Foreigners could argue, with considerable justification and from premises the opposite of those advanced by Field, that American commercial policy was selfish, naval policy was coercive, missionary policy was meddlesome and insulting, and the general missionary spirit was crude and exploitative rather than uplifting.

To be effective, a book of this kind should be intercultural and free of strong national and religious biases. This one is ethnocentric. Its virtues lie in its grand design, not in its execution, in its telling something of a neglected though disconnected story, and in its groundbreaking research.

University of California, Santa Barbara

ALEXANDER DeCONDE

THE CREATION OF THE AMERICAN REPUBLIC, 1776-1787. By *Gordon S. Wood*. (Chapel Hill: University of North Carolina Press for the Institute of Early American History and Culture. 1969. Pp. xiv, 653. \$15.00.)

At first glance, this weighty volume appears to be an attempt to revive, in different guise, the class conflict, revolution-counterrevolution interpretation of the American Revolution and Constitution made popular some half century and more ago by the old Leftists: Becker, Beard, Schlesinger, Parrington, and other Progressives. Like Becker, Wood insists that the conflict was more than a question of home rule; like Becker also, Wood builds his thesis on an elitist rather than on a democratic society. What began as a protest against Parliament escalated into "a genuine revolutionary movement" (the Schlesinger thesis), a radical republicanism based on radical Whiggism in which a virtuous people would subordinate the good of the individual to the greater good of the whole. Thus, the Revolution was "one of the great utopian movements in American history." In the end, however, this radical republican revolution was subverted by an aristocratic, social counterrevolution, not by large owners of personal property as Beard contended but by a natural aristocracy of intelligence, ability, ambition, and accomplishment. Using the rhetoric of democracy, they accomplished their aims of aristocratic domination by enlarging the arena of republican government from a local to a national basis, so that only the socially prominent or widely known men could win elections, and by basing their system on selfish human motives.

The difficulty with such a thesis is that Wood, like Becker, Beard, Schlesinger, and others, undermines his own interpretation by his own statements as well as his evidence. For example, while contending that the Revolution was no simple colonial rebellion against British imperialism but was meant to be a profound social revolution, he contradicts himself with the statement that for revolutionary Whigs the problem of British authority had become the single problem of colonial politics, and the English Crown and imperial system had come to stand for all that was wrong with American society. Later he enforced the contradiction with evidence that "in fact, the Revolution had begun precisely because the English, by 'declaring them-

selves invested with power to legislate for us in all cases whatsoever,' had threatened the Americans' very existence as a free people."

Wood uses Pennsylvania as the prototype of his social revolution, but here again the contradictions are particularly glaring. Pennsylvania, he said, saw the most abrupt and complete shift in political power, wrote the most "radical" constitution, and had the most extreme ideology. But Pennsylvania, where the revolution "had become a class war between the poor and the rich, between the common people and the privileged few" (Becker and others), was only an extension and an exaggeration of what was happening elsewhere in America. On second thought, however, Wood says that Pennsylvania hardly experienced a violent social convulsion and had long been a symbol of freedom and egalitarianism. In fact, the Revolution in Pennsylvania "could scarcely be regarded as a rising of the masses against the few," grievances expressed in pamphlet and press did not go deep into the society, the internal revolution was very much a minority movement, and radicals "actually feared the traditional deference of the people to their established leaders."

Republicanism fares no better than does Pennsylvania in Wood's account. Far from being something new and radical, it was, as Wood's evidence shows, so well understood, so taken for granted, so much a part of Americans' assumptions about politics that few felt any need to explain its origins. Wood also quotes Jefferson to the effect that Americans threw off monarchy and took up republicanism with as much ease as they would discard old clothes for new. In fact, Wood could have found ample evidence that when the historian scrapes off the veneer of British imperialism in 1750, he finds republicanism (or middle-class democracy) in full bloom, with Americans often being accused of desiring to discard monarchy and embrace republicanism.

It is unfortunate that Wood has allowed a dubious thesis to obscure the really excellent sections of his book. If his history is faulty, his discussions of political theory and political practices (divorced from social revolution) are well worth reading. But on the strength of Wood's own statements and evidence, one could construct a democracy or egalitarian colonial society, a revolution to preserve this liberal society from British imperialism, an easy transition to a republicanism already present, and a popularly based Constitution that was adopted in a democratic society and "embodied what Americans had been groping towards from the beginning of their history."

Michigan State University

ROBERT E. BROWN

TIMOTHY PICKERING AND AMERICAN DIPLOMACY, 1795-1800. By *Gerard H. Clarfield*. [International Relations Series.] (Columbia: University of Missouri Press. 1969. Pp. x, 233. \$6.00.)

PROFESSOR Clarfield has written a book at once competent and disappointing. While it is a gracefully written and carefully documented account of the actions and ambitions of Pickering as Secretary of State, it adds little to our understanding of the operations and motivation of American diplomacy in the administrations of Washington and Adams. Though obviously the work of a well-trained scholar, this study suffers from the author's distaste for the Anglophilia of his subject, his inability to indicate how Pickering's prejudices significantly influenced the course of international relations, and his ambivalent stance respecting the French diplomacy of John Adams.

Clarfield entertains a sharp distaste for Pickering's personality and political philosophy. Obviously it is not necessary for a student of Pickering to be an eighteenth-century conservative; it is useful for a biographer to be less than eager to suspect the

worst of his subject. Pickering's concern for the friendship of England was perhaps excessive, but advocacy of an alliance policy is not of necessity less patriotic than allegiance to nonentanglement. Implications that Pickering paid insufficient attention to the "national honor" and was prepared to descend from obstructionism to "conspiracy" are neither clarified nor proven.

Yet if Pickering is not indulged, he is awarded undue importance. Clarfield's central failure is his inability to prove that importance. In part this failure is a tribute to his scholarship. Clarfield's research in the diplomatic correspondence of the period sustains the accustomed belief that neither Washington nor Adams relied on the opinions of Timothy Pickering in shaping the major diplomatic policies of their administrations. Consequently, Clarfield's painstaking concern to show how Pickering's prejudices influenced his own foreign policy views is somewhat irrelevant even when persuasive. Pickering's ideological antagonism toward France did not, for example, significantly influence John Adams' decision to send a second peace mission to Paris.

It is Adams' decision to seek to end the naval war by means of diplomatic negotiation that furnishes the climax of Clarfield's narrative. His analysis of "Adams' decision for peace" suffers from an unacknowledged uncertainty respecting the President's vision and motives. For the most part Clarfield tends to follow the thesis of Stephen Kurtz and to contrast the pacific vision of Adams with the warmongering designs of his cabinet. Yet certain of the author's footnotes take cognizance of the fact that Adams in the summer of 1798 was as ready for war as Hamilton, McHenry, or Pickering and that Adams was himself primarily responsible for the delayed departure of the peace mission. Adams' final judgment in October 1799 to override the advice of Pickering and dispatch the commissioners forthwith is praised by Clarfield, but its motivation appears to elude him. At one point he implies that it was the product of Adams' practical grasp of international realities; at another point he seems to feel it was at least indirectly connected with the triumph of the Jeffersonians in the Pennsylvania elections and Adams' fears respecting the presidential election of 1800.

Clarfield has not contributed significantly to our understanding of the general themes of American diplomacy in the Federalist period; he has provided a convenient summary of the more important words and actions of Pickering as Secretary of State.

Lafayette College

RICHARD E. WELCH, JR.

THE COLOR PROBLEM IN EARLY NATIONAL AMERICA AS VIEWED BY JOHN ADAMS, JEFFERSON AND JACKSON. By *Frederick M. Binder*. [Studies in American History, Number 7.] (The Hague: Mouton. 1968. Pp. 177. 24 gls.)

You may wish to run through some of the changing opinions on Negroes and Indians of Adams, Jefferson, and Jackson, with something added respecting John Quincy Adams and a few others. If so, this book will provide data on the subject. The book looks thin and pale indeed set beside such writings as Winthrop D. Jordan's *White over Black* (1968) and Marion L. Starkey's *The Cherokee Nation* (1946), for instance. But, theoretically, being much less complex and probing, the Binder volume should be a more handy tool, a gathering of "facts." Unfortunately, it shares the fault of such monographs in that it lacks a dimension to give substance to those "facts." Thus, the author is sympathetic to Indians and Negroes, but his awkward

approach leads him to criticize federal policy in such terms as: "Were not [the Indian wars] caused by what the Indians at least considered unfair pressure to surrender their lands?" The "pressure" had many aspects, fair and unfair, with which Americans as citizens and as historians have struggled. Binder's monograph does not struggle. He is hard on Adams, but for obscure reasons chooses to be tolerant of Jackson: "a kind yet firm master," and a responsible nationalist. The monograph elsewhere picks and rejects information along undefined lines. Thus, its bibliography indicates, among other matters, awareness of the Sally Hemming problem in Jefferson's life, but the text does not. Binder's "factual" approach may please a few treadmill history teachers and their students. But, its implicit liberalism notwithstanding, it is obsolete in concept and execution.

Antioch College

LOUIS FILLER

THE ERA OF EXPANSION: 1800-1848. By *Don E. Fehrenbacher*. [American Republic Series.] (New York: John Wiley and Sons. 1969. Pp. ix, 165. Cloth \$6.50, paper \$2.50.)

THE NEW INDUSTRIAL SOCIETY. By *Bernard A. Weisberger*. [American Republic Series.] (New York: John Wiley and Sons. 1969. Pp. ix, 162. Cloth \$6.50, paper \$2.50.)

THESE two slender volumes are part of a seven-volume series covering the whole span of United States history. According to the editors, the series is "designed to be illuminating rather than comprehensive," limiting "its presentation of factual details in order to make room for fuller explanation of major events and trends." Both of these volumes execute the design. Indeed, they do much more than the editors promised. Their "explanations of major events and trends" are really rather admirable syntheses of the scholarship on these events and trends. One is continually aware of the scholarly competence of both authors, of how much "on top of" the literature on their period of specialty they are. This is apparent long before one arrives at the historiographical essay that concludes each volume. Yet these essays certainly cannot be passed by without special notice. They are in the best tradition of historiographical writing, linking historians and their interpretations of the past with their cultural context and organizing the literature into comprehensible patterns.

Any proper appreciation of the craftsmanship involved in these two volumes must take account of the severe limitations of size, presumably a reflection of the high cost of publication today. In order to accomplish their objective within a roughly seventy-thousand-word allotment for a half century of history, both authors have opted to economize on details rather than to exclude entirely events and trends that have hitherto been regarded as of major significance by historians generally. The result is that neither volume really has a single thesis or even an overriding interpretation that is offered to explain the half century being surveyed. While the authors are certainly not without opinion on the relative importance of the events and trends with which they deal, their volumes are not heavily interpretive. What emerges is rather a series of syntheses of the best scholarship on a number and a variety of major events and trends that took place during each of these periods.

To define the audience for which these volumes were intended is not an easy undertaking. On one end of the scale, one must conclude that they assume a degree of familiarity with the factual details of United States history. On the other end, it seems equally certain that they are not written for historians. Probably, between these

extremes, there is a considerable audience of "advanced students" of American history for whom a brief, clear, and highly competent treatment will be a boon.

Tulane University

W. BURLIE BROWN

LEWIS AND CLARK: PIONEERING NATURALISTS. By *Paul Russell Cutright*. (Urbana: University of Illinois Press. 1969. Pp. xiii, 506. \$12.50.)

THIS is a difficult book to review using normal canons of historical scholarship. Much of it is personal and discursive, the loving effort of a modern biologist to rescue the reputations of Meriwether Lewis and William Clark, particularly the former, as naturalists. Almost from the beginning, those reputations have suffered owing to the tragic death of Lewis, Benjamin Smith Barton's failure to give a scientific account of the expedition, and Nicholas Biddle's inability, as first editor of the journals, to give much attention to natural history. A number of newly discovered plants did enter scientific nomenclature by way of Frederick Pursh, who carried many of the specimens with him to London, and some of the animals similarly entered the stream of scientific knowledge through Alexander Wilson and George Ord, who worked from Lewis and Clark specimens in the Peale Museum. Unfortunately, though, many of the species the expedition encountered had to be "rediscovered" by later naturalists who followed in its path.

The major part of the book is given over to a narrative of the expedition, with an emphasis on natural history broadly defined. Although the author is a zoologist, he does not confine himself to that area as did Raymond D. Burroughs in his *The Natural History of the Lewis and Clark Expedition*. A "Summary of Discoveries" follows each chapter, including plants, animals (both of which are treated fully in appendixes), Indian tribes, and topographical features encountered during particular segments of the journey. Interrupting the story, but in a way that makes the book far more attractive, are lessons in botany and zoology, discussions of the efficacy of early nineteenth-century medical practices, injunctions against the further damming and "improving" of western scenic resources, and accounts of the author's retracing of the routes of the expedition.

In many ways, the most interesting chapter in this book is placed at the end of the narrative. In it, Cutright examines the fate of the zoological, ethnological, and botanical materials gathered by the explorers. While it is, in the main, a sad story, it should be read by historians interested in American science and museums in the nineteenth century; its bleakness is relieved somewhat by the continued existence of a substantial Lewis and Clark herbarium at the Academy of Natural Sciences in Philadelphia, a collection that Cutright helped to reassemble.

The University of Illinois Press can take pride in this handsome, if costly, volume, which is a worthy successor to their earlier publication of Donald Jackson's edition of Lewis and Clark letters. While the book does not entirely dispel questions about the accomplishments of the expedition, it does add a significant dimension to a familiar story. The failure, if such it was, must be assigned to republican science and not to the leaders of this first great national exploration.

University of Delaware

GEORGE F. FRICK

PROTESTANT AMERICA AND THE PAGAN WORLD: THE FIRST HALF CENTURY OF THE AMERICAN BOARD OF COMMISSIONERS FOR FOREIGN MISSIONS, 1810-1860. By *Clifton Jackson Phillips*. [Harvard East Asian Monographs, Number 32.] ([Cambridge, Mass.:] East Asian Research Center, Harvard University; distrib. by Harvard University Press, Cambridge, Mass. 1969. Pp. viii, 370. \$3.50.)

JOHN K. Fairbank's presidential address to the American Historical Association in 1968 pleaded for a new understanding of China as the "Assignment for the '70's" (*AHR*, LXXIV [Feb. 1969], 861-79). Fairbank remarked on one disaster area that prevented such understanding: the general ignorance of missionary history. The missionary "seems to be the invisible man of American history," and religious expansion is treated as "a specialized subsector of the American experience, not as noteworthy as economic and political expansion."

Clifton Jackson Phillips' doctoral dissertation, completed in 1954, was designed to make the missionary more visible. The mode of publication, in a rather unattractive photographic reproduction of the typescript, probably dooms the missionary and the book to semivisibility, back in their private subsectors. This is unfortunate, for, if it were widely circulated and read, Phillips' study could do more than most to inform the historical community about the roots of racism, imperialism, expansionism, and other isms being decried today.

The story deals with the American Board of Commissioners for Foreign Missions and its work from 1810 to 1860. The missionaries may well have been guilty of contributing to the isms described above, but not ordinarily in the context in which latter-day critics place them.

Phillips writes of the missionaries without evident bias, despite the fact that these seldom self-critical romantics have long been ripe subjects for caricature. Indeed, they have usually been visible pre-eminently in cannibal cooking pots, in cartoons. Missionary historians usually choose between concentration on foreign fields or on the sending center. Phillips tries both. At the moment when his travelogue becomes a bit wearying—there are just a few too many hops, skips, and jumps; there is too rich an array of proper names—he comes back to America to write several reflective historical essays on the meaning of it all.

Concentrating on ABCFM files and other Anglo-American sources, the author is not able to give as full and balanced a picture as he could have had he been able to penetrate the literature of the missionized countries, but he handles the domestic materials conscientiously. The book could have been improved had he concentrated in more detail and with greater depth on the questions of motivation and of the theological context. A secular age has difficulty experiencing empathy for these self-confident religious and cultural imperialists: men of gentle spirit, full of compassion, who paradoxically resorted, as Phillips points out, to strategies and metaphors of conquest. At the center of their vision was a postmillennial picture: they were to help prepare the world for the return of Christ; they never doubted that they would be successful.

University of Chicago

MARTIN E. MARTY

HOUSE OF REFUGE: ORIGINS OF JUVENILE REFORM IN NEW YORK STATE, 1815-1857. By *Robert S. Pickett*. [New York State Study.] ([Syracuse, N. Y.:] Syracuse University Press. 1969. Pp. xix, 217. \$7.50.)

AFTER long neglect, the history of juvenile delinquency and its treatment is beginning to receive the scholarly attention it merits. The groundbreaking insights of such students as Robert Bremner and Roy Lubove will soon be augmented by the work of younger men like Michael Heale and Jack M. Holl, and the Charles Warren Center at Harvard is planning a three-volume edition of primary documents on child welfare in America. Robert S. Pickett's study of the New York House of Refuge during its formative years is thus an additional sign of recent activity in its general area of concern. This institution, the first in the United States to deal specifically with juvenile offenders, opened its doors in 1825 and by the mid-nineteenth century had become the largest establishment of its type in the world. Its early history forms an important chapter in the development of social welfare in the ante bellum period.

The significance of his topic makes it all the more unfortunate that Pickett's book is a disappointing performance. Strangely, though chairman of the department of family relations and child development at Syracuse University, he does little to make explicit use of the understanding that modern social sciences might have contributed to his subject. Nor does he demonstrate adequate familiarity with the secondary literature on American philanthropy and social reform; such a background would have enabled him to place his data within a more convincing historical context, and he would not have found so strange the blend of private and public interaction manifested in the management of the Refuge. This is, in fact, quite typical of American reform. Often Pickett leaves the reader hanging for want of sufficient explanatory detail as when he expects the reader to understand a confrontation between Governor Seward and officials of the Refuge without adequate knowledge of what was said in the correspondence involved. Important subjects are left unexplored: despite placing great emphasis on the work required of inmates at the Refuge, for example, the author gives little description or analysis of its industrial program. Sometimes the manuscript is unintentionally misleading: one would never know, for instance, that in 1842 the New York legislature did pass an important law affecting contract labor among convicts, though not so strict a one as the proposal mentioned by Pickett on page 136. Finally, despite apparently self-conscious attempts to enliven the book with semianecdotal case materials and occasional hyperbole, the writing abounds in awkward transitions and other evidences of laborious composition.

To his credit, Pickett has recognized a job that needs to be done, provided some insight into the way in which support can be mustered and sustained for a social experiment, and mapped out a terrain that later students can follow as they amplify and supplement his work. He has also recognized how many early child welfare reformers erred by attaching too much importance to the vices of the individual misdemeanant and too little to the deficiencies of the social order in which he lived. Though flawed, this work does have merit, and it should serve a useful purpose in a field now suffering from a paucity of available studies in print.

State University of New York, Buffalo

W. DAVID LEWIS

THE STORY OF THE GENERAL THEOLOGICAL SEMINARY: A SESQUICENTENNIAL HISTORY, 1817-1967. By *Powel Mills Dawley*. (New York: Oxford University Press. 1969. Pp. xvii, 390. \$7.50.)

It is a mark of the high quality of this book, and especially of Professor Dawley's work in the immediate sources, that a reader gains some sense of being a direct observer. The feeling, more precisely, is that of standing in a crowded Manhattan street and viewing, over a very high wall, a self-contained and somewhat elegant drama. The author, a veteran professor of ecclesiastical history at General Seminary, might well have done more to integrate his subject with developments outside the Anglican communion, but the sad fact seems to be that this official seminary of the Episcopal Church was indeed oblivious, through much of its history, to its intellectual and social surroundings.

The seminary's name expressed an aspiration never fully realized. For the first ten years, in fact, the school was caught in a cross fire between those who wished a truly "general" institution and others, headed by Bishop Hobart of New York, who preferred either diocesan seminaries or else a nominally national institution that in reality would be under local control. Harassed during their first New York sojourn by the resourceful Hobart and his sympathizers, the seminarians fled that "scene of bustle and wickedness" for the steadier habits of New Haven. But the will of one Jacob Sherred suddenly offered a large benefaction which seminary and Hobartians had either to contend over in the courts or enjoy together in Manhattan; by 1827 the school was installed in its permanent location at Chelsea Square on the West Side.

After this point in the narrative, Dawley is increasingly obliged to record house-keeping information: physical details of buildings and their alterations, curricular data, biographies of faculty, and vignettes of student life. Though these matters are handled ably, the general historian will be more interested in Dawley's analysis of the seminary's stimulating, if nearly ruinous, experiences with the Tractarian and Ritualist controversies of 1840-1880.

His account of the late nineteenth century, the seminary's Gilded Age, is more than a little depressing. In 1878 the institution acquired a wealthy dean, Eugene Augustus Hoffman, whose promotional abilities and substantial pump priming set a "golden stream" flowing into Chelsea Square. In view of the acknowledged intellectual somnolence of the school in this same era, and in view of the lack of social concern implied by Dawley's meager attention to anything remotely suggesting a social gospel, the encomiums for Hoffman seem subject to more serious reservations than the author brings himself to express.

Dawley attempts in his last eighty pages to chronicle the entire complex period since 1893, and the narrative inevitably falls into annual reportese. ("Miss Chapman, the dietitian . . . rose to meet every new emergency with characteristic cheerfulness and competence.") Sesquicentennials, while long enough in coming, have a way of closing in rapidly on official historians, especially those as conscientious as the author of this book.

Harvard University

WILLIAM R. HUTCHISON

AN AMERICAN CONSERVATIVE IN THE AGE OF JACKSON: THE POLITICAL AND SOCIAL THOUGHT OF CALVIN COLTON. By *Alfred A. Cave*. [Texas Christian University Monographs in History and Culture, Number 5.] (Fort Worth: Texas Christian University Press. 1969. Pp. xi, 69. \$3.50.)

CALVIN Colton thought he was a republican defender of liberty who lived during the reign of the Bad King, Andrew Jackson. He worked tiresomely to unseat "The Usurper," and to replace him and his minions with the republican "Pretender," Bonnie Prince Hal of Kentucky. He failed.

Beyond that there really is not much to be said about Colton, and Alfred Cave has confined his analysis of this Whig publicist and propagandist to extended essay length, an economy stemming only partly, I think, from the fact that Colton's private papers have not survived. New England-born, Colton tried the ministry for awhile, lived in England where he wrote an explanation of *The Americans* and a manual for emigrants, edited a New York newspaper, and served as Clay's official biographer and press agent. In the process he became increasingly conservative (in religion he shifted from the Presbygationalism of New York's Burned-Over District to Episcopalianism), but in public he stifled his "the-people-are-a-great-beast" notions in favor of coonskin demagoguery. He was neither an effective nor a slashing editorialist; nor were his books on national character or political economy particularly penetrating. Cave confines himself to summarizing and commenting on Colton's writings themselves and on their importance for Colton's development, but he does not claim significance for them in influencing other Whig thinkers and tacticians. Cave accepts Joseph Dorfman's dismissal of Colton on economics as a tedious mediocrity. On humanity, Colton thought that slavery was wrong and that American slaves were well off. In the end there is little left but Cave's injunction that Colton clearly expressed the conservative notion that "capitalism might conceivably make democracy safe for America."

University of California, Los Angeles

FRANK OTTO GATELL

THE JACKSONIAN ECONOMY. By *Peter Temin*. [The Norton Essays in American History.] (New York: W. W. Norton and Company. 1969. Pp. 208. \$5.50.)

THIS slender but excellent study is a valuable addition to that large body of monographic literature on the Jackson era that has accumulated since publication of the provocative *Age of Jackson* by Arthur Schlesinger, Jr. As a result, long-accepted assumptions regarding the nature of Jacksonian democracy, and especially the role of Jackson in shaping the major developments of the era, have been challenged, radically modified, or totally rejected.

Temin is an economist, and, skillfully exploiting the methodology and the analytical skills of his discipline, he has undertaken to reappraise some of the highly dramatic and controversial developments of the era. He focuses on the economic gyrations of the period: the "destruction" of the Second Bank of the United States and the inflation and expansion that followed, with the subsequent panic and depression that afflicted the nation. Many of Jackson's contemporaries and a number of later historians attributed that erratic performance of the nation's economy to the prejudices and mistaken policies of the strong-willed general who occupied the White House. Emphatically Temin rejects this thesis. "Jackson's economic policies," he asserts, "were not the most enlightened the country has ever seen, but they were by no means

disastrous. The inflation and crises of the 1830's had their origin in events largely beyond Jackson's control and probably would have taken place whether or not he had acted as he did. The economy was not the victim of Jacksonian policies; Jackson's policies were the victims of economic fluctuations."

His thesis is clear: Jackson was no villain; his policies were not the cause either of the inflation or of the panic of 1837. Both were the results of developments outside of the United States and beyond the control of the nation's leaders. A good harvest in England in 1832 and an economic boom there released capital for export to the United States. Simultaneously there was a phenomenal expansion in the opium trade to China, accompanied by a decline in the demand for specie there, which resulted in a corresponding rise in the supply of specie in the United States. Both developments supported the inflationary tendencies in the American economy. "A diminution in the capital flow from England to America," argues Temin, "was the force that led to the crisis [the panic of 1837]."

To be sure, this book offers much more and is especially good for the general historian. The author's argumentation is forceful, convincing, and supported by much sophisticated statistical analysis. Yet his exposition is clear and free from abstruse mathematical formulas. Within the limits he has set for this essay, Professor Temin has done a masterful job.

California State College, Hayward

WILLIAM A. SULLIVAN

JOHN A. JOHNSON: AN UNCOMMON AMERICAN. By *Agnes M. Larson*. (Northfield, Minn.: Norwegian-American Historical Association. 1969. Pp. viii, 312. \$6.50.)

THE theme of the late Professor Larson's biography of Johnson centers upon his "many-sided career in business, politics, and cultural life." Thus, the study surveys Johnson's surmounting straitened circumstances as an immigrant in mid-nineteenth-century rural Wisconsin and traces his progress through a successful business career. In business, Johnson's outstanding achievements occurred in the manufacture of farm implements and machine tools.

This was not all. Johnson possessed a wide-ranging interest in public affairs; as a young man, he entered politics serving as Dane County clerk and in the Wisconsin legislature. Dedicated to democracy, Johnson loathed slavery, and in 1857 he supported a move to give black Wisconsinites the ballot. Concerned for the public weal, he took the long view. He favored effective, not repressive, regulation of Wisconsin railroads. He was protectionist on tariff policies, but not unqualifiedly so. Believing in sound fiscal management, Johnson advocated banking reforms including guarantee of deposits, and he opposed free silver. "Our great need is safe banks and safe currency," he wrote.

Though firmly devoted to American ways, Johnson cherished his immigrant background. His feeling for Scandinavian culture was steadfast, and, although he forbade his children to speak Norwegian at home, Johnson promoted the establishment of courses in Scandinavian at the University of Wisconsin. As a contributor to Norwegian-American newspapers and author of a guidebook for aiding them to become farmers, Johnson enjoyed prestige among Norwegian immigrants. Whatever his relationship, he believed immigrants should adopt those principles basic to American democracy.

Drawn from various sources, including business records, trade journals, news-

papers, and Johnson's writings, this study contains much useful information, particularly for students of immigrant groups in America and for business historians. It has a place in Wisconsin history. The index is adequate, but there is no bibliography, and readers may question the value of certain appended documents. Occasionally, inconclusive statements and digressions from the narrative occur. Larson, nonetheless, has placed in perspective the career of John A. Johnson, immigrant businessman.

Wisconsin State University, Oshkosh

EDWARD NOYES

WILLIAM WELLS BROWN: AUTHOR & REFORMER. By *William Edward Farrison*. [Negro American Biographies and Autobiographies.] (Chicago: University of Chicago Press. 1969. Pp. xii, 482. \$12.50.)

HENRY OSSAWA TANNER: AMERICAN ARTIST. By *Marcia M. Mathews*. [Negro American Biographies and Autobiographies.] (Chicago: University of Chicago Press. 1969. Pp. xvii, 261. \$9.50.)

A VALUABLE product of the interest in black studies has been the recovery from obscurity of historically significant black Americans. These two biographies are cases in point.

William Wells Brown, fugitive slave, was a prominent abolitionist whose slave narrative was among the first of its kind and one of the most popular. Self-taught, he became an eloquent and influential lecturer. He was also the first American Negro to write a novel, he pioneered black drama, and he attempted historical writing—all in the cause of abolition and equality. He also presented significant firsthand reports of life among American black refugees in Canada.

Despite his talents, Brown never attained the stature of Frederick Douglass as a leader. This may have been because of character traits that this biography hints at but does not develop; or, it may have been the result of Brown's erratic responses to basic issues confronting his people. Long an opponent of emigration, he lent himself to James Redpath's questionable scheme of Haitian settlement, then he reversed himself, and, after Reconstruction, he changed again, feeling that emigration would cause the white South to treat remaining Negroes better. There were similar contradictions in Brown's attitude toward black participation in the Civil War.

This is the most complete biography of Brown available, and it deserves a place in every college and university library. Unfortunately, its usefulness is diminished by such a painstaking concern for each detail of Brown's life that the context of his times is often obscured, which means in turn that the nonspecialist reader may miss the full significance of some of Brown's changes in outlook.

Unlike Brown, who never ceased to be involved with his own people, Henry Tanner sought to escape the bitterness of American black experience through art and expatriation. Born in 1859, he was the son of an African Methodist minister and for some time a pupil of Thomas Eakins. Tanner served a long, discouraging apprenticeship as a painter in a society notoriously indifferent to its own artists and especially to those of dark skin. Tanner fled to Paris, where, by the turn of the century, he had gained such a reputation that he won the friendship and patronage of wealthy white Americans. Nor was other recognition lacking. Edward Bok reproduced some of Tanner's painting in *The Ladies' Home Journal*, and *The World's Work* published his autobiography. Doubtless a factor in Tanner's acceptance in the United States, apart from the acclaim given him abroad, was that his work, largely devoted to Biblical themes, had little relevance to black life in the United States.

Even in the acceptance, there was gall, however. Few critics or reporters failed to call attention to Tanner's being Negro, suggesting that he was a good Negro painter rather than simply a good painter. Tanner's response sums up the ambivalence of many American blacks toward their country: "this condition has driven me out of the country, but still the best friends I have are 'white' Americans and while I cannot sing our National Hymn, 'Land of Liberty,' etc., still deep down in my heart I love it and am sometimes sad that I cannot live where my heart is. . . ."

The major flaw of Mrs. Mathews' work is the large mass of verbatim correspondence interlarded throughout. Much of it is trivia and could well have been condensed. The book should enjoy a wide audience, however, for its intrinsic interest and its portrayal of how one black fared in self-imposed exile. If Tanner sensed any of the "extraordinary perils . . . which the American Negro encounters in the Old World," about which James Baldwin wrote, it does not emerge from this record.

Lewis and Clark College

ROBERT CRUDEN

FORGING A MAJORITY: THE FORMATION OF THE REPUBLICAN PARTY
IN PITTSBURGH, 1848-1860. By *Michael Fitzgibbon Holt*. (New Haven, Conn.:
Yale University Press. 1969. Pp. ix, 408. \$10.00.)

Using Pittsburgh as a case study, the author has employed the sources and techniques of quantification to discover specific economic and social influences on the politics of the 1850's, an approach he claims earlier historians have neglected. Holt selected Pittsburgh because that city polled a larger percentage of the Republican vote in 1860 than any metropolis in the nation, because its medium size and the stability of its population adapted it to statistical analysis, because it had a large foreign population, and because its economy was rapidly changing. The author presents the story chronologically and at each successive stage examines the leadership of the local parties, analyzes the relevant content of national, state, and local issues, gives comparative ratings of influence to them, and tries to discover what issues appealed to what voters by city ward, by economic status, by vocation, by nationality, by religion, and by party.

Holt concludes that Republicans in Pittsburgh cared little about sectionalism or slavery in the 1850's and paid scant attention to the rhetoric of national party leaders and platforms, which formed so large a part of the traditional history of this era. The major forces that created the Republican party in Pittsburgh were anti-Catholicism, anti-foreignism, and the antirailroad impulse arising from municipal oversubscription to railroad bonds. Using these issues skillfully, Republican leaders allied with the Know-Nothings and supplanted the traditional Whig hegemony of the city. The moral crusade against slavery and fear of an aggressive slavocracy played a secondary and minor role. This conclusion supports Holt's hypothesis that "the elements which go into political coalitions are often small and local, not broad and national."

The book contains an appendix of sixty-one statistical tables that represent a prodigious amount of work in the manuscript census returns and other hard-to-use, and equally hard-to-check, sources. In these tables the condensed data on population, wealth, vocation, religion, voting, and other topics are further refined into percentages and calculations of correlation. The author explains his quantitative techniques in a second appendix. He wisely uses his statistical material with caution and restraint, recognizing that it is suggestive rather than compelling evidence. The book has a bibliography and a brief index which, unfortunately, omits many of the social or economic terms that the book emphasizes such as labor, merchant, poor, wealthy, middle class, coal, liquor,

German, Irish, and the like. On the whole, the book offers new insights and opens a mine of new information on a period still seen too commonly through the glasses of sectional propaganda.

Pennsylvania State University

PHILIP S. KLEIN

THE PEOPLE IN POWER: COURTHOUSE AND STATEHOUSE IN THE LOWER SOUTH, 1850-1860. By *Ralph A. Wooster*. (Knoxville: University of Tennessee Press. 1969. Pp. xi, 189. \$6.25.)

IN his examination of the "outer structure" of government in the seven original states of the Confederacy, the author devotes a chapter to each of the three branches of government, one to the counties, and one to a summary called "Close of an Era." A considerable portion of each of the first four chapters deals with constitutional provisions concerning voting and officeholding from the beginning of each state. Most of the statistical material deals with the last pre-Civil War decade.

The discoveries and conclusions fall a little short of startling: South Carolina was the most highly centralized and least "democratic" of the states, while Mississippi was the most "democratic"; legislators were generally middle-aged farmers, planters, and lawyers, the great majority of whom were born in the slaveholding states, owned property, and held office for a brief period; the number of slaveholders and planters in government increased during the 1850's; county government was "democratic" in all states except South Carolina; there was no great difference in the amounts of property held by Whigs and by Democrats; gubernatorial power was severely limited in most states; most of the governors in the late ante bellum period were "prominent political and social leaders in their states."

Wooster had to spend many hours working with the federal manuscript census schedules of 1850 and 1860, registers of state and county officials, legislative journals, county court minutes, and other materials to compile the information for the ten tables in the text and the fifty-nine in the two appendixes. Tables in the first appendix show the ages, places of birth, occupations, real and personal property holdings, slaveholdings, and slaveholdings by political factions of the legislators of the seven states. The second appendix is less comprehensive in depicting these "personal characteristics" for members of some of the county governing boards. The tables in the text mainly summarize those given in the appendixes.

After reading the short text and examining the tables, one feels compelled to ask what this material means. Were the actions of the state and local officials of South Carolina noticeably and measurably different from those of their counterparts in Mississippi? Did the new political party structure of the Jacksonian period produce a changed relationship between state and local officials? Did it matter that senators held more property and were two or three years older than representatives? Or that county officials held less property and were older than state officials? Or that an individual was born in the upper or lower South? Or outside the South? These and many other questions that might properly be raised will remain unanswered until this work becomes the source book (as it is claimed that it will) for future studies on the "internal governmental structure" of these states.

Indiana University

CHASE C. MOONEY

LINCOLN'S RAILROAD MAN: HERMAN HAUPT. By *Francis A. Lord*. (Rutherford, N. J.: Fairleigh Dickinson University Press. 1969. Pp. 325. \$10.00.)

By any criteria one cares to choose, this is a very disappointing book. Its thesis is overstated and unfocused, its organization is terribly muddled, the research is sorely inadequate, and the writing is awkward, disjointed, and repetitive.

It is disheartening to make this evaluation because the career of Herman Haupt unquestionably provides several fertile areas for study. An enigmatic, controversial personality, Haupt made invaluable contributions to the northern war effort. As superintendent of military railroads, he undertook the formidable task of organizing coherent transportation and construction operations for the Union Armies. Through his unyielding efforts, every eastern commander from McDowell to Meade received a steady stream of supplies and reliable transportation facilities despite a host of problems that would have overwhelmed a lesser man. He was also an astute military observer who frequently supplied Lincoln, Stanton, and other key officials with accurate intelligence information. A brilliant engineer, he wrote two important treatises on bridge construction and turned out numerous practical inventions for military use.

Lord's account contains a lot of useful information and helps to disclose Haupt's contributions, but wading through his tangled narrative becomes a burdensome chore for the reader. The introduction serves as little more than a preview of coming attractions. The individual chapters fall between the stools of topical and chronological organization and therefore constantly repeat episodes, incidents, and details, sometimes nearly verbatim. To cite but one of many examples, Lord makes the point that Daniel C. McCallum, Haupt's administrative superior, totally ignored Haupt in his final report no fewer than three times. A related episode, and its supporting document, is repeated twice. This redundancy is compounded by Lord's tendency to skip around in his narrative to the point that the reader despairs of ever piecing the story together.

The gaps in research are equally serious. Not only does Lord claim too much for his subject, but he advances the broadest of generalizations (repeating them several times) on the skimpiest of evidence. Often he resorts to sheer intuition, and sometimes the evidence he presents seems to contradict the point he is making. He admits that the main sources for his study are the Haupt, Stanton, and Lincoln Papers, Haupt's *Reminiscences*, and the *Official Records*. The bibliography is practically barren of standard biographies, monographs, and other pertinent recent literature. This results in some gross oversimplifications of interpretation. For example, he treats Stanton harshly and concludes that "it is entirely possible that the man was not mentally well at times." But there is no indication that he is even familiar with the Thomas-Hyman biography of Stanton or even Fletcher Pratt's study.

Lord seems largely to have adopted Haupt's interpretation of men and events as given in his *Reminiscences*. Limitations of space prevent any real cataloguing of the errors that result, but the reader can compile his own list at leisure. As for style, suffice it to say that Lord has a fatal fascination for irrelevant detail and digression.

University of Rhode Island

MAURY KLEIN

SAMUEL FRANCIS DU PONT: A SELECTION FROM HIS CIVIL WAR LETTERS. Volume I, THE MISSION: 1860-1862; Volume II, THE BLOCKADE: 1862-1863; Volume III, THE REPULSE: 1863-1865. Edited by *John D. Hayes*. (Ithaca, N. Y.: Cornell University Press for the Eleutherian Mills Historical Library. 1969. Pp. cxvii, 425; xxviii, 553; xxviii, 562. \$45.00 the set.)

THESE ably edited volumes of the Civil War correspondence of a Union naval commander must be consulted by historians of that conflict. After forty-five years in the navy, nearly half of which had been spent at sea, Admiral Du Pont immediately placed his skill and experience at the disposal of the Union when the war broke out.

After a short stint in Washington, Du Pont was placed in command of the highly successful Port Royal expedition. After running past the forts, which were then promptly evacuated by the Confederates, Du Pont landed the army force under General Thomas W. Sherman. At Port Royal the plight of the Negroes, who had been abandoned by their masters, was so distressing that Du Pont, who had been an apologist for the institution of slavery, became convinced that the South had misrepresented slavery and that the "peculiar institution" was cruel and wicked and must end.

After the fall of Fort Pulaski, the ease with which it and Port Royal had been taken was misconstrued in Washington by the Navy Department. Fox, the Assistant Secretary, insisted that Du Pont and the navy alone, using mostly ironclad monitors, should reduce the Charleston forts and other defenses. Fox wanted Charleston captured in April 1863, about two years after the firing on Fort Sumter. Such a success, Fox believed, would have important political and moral repercussions since Charleston symbolized secession.

Unfortunately, what Fox proposed was not a run past the forts nor the reduction of an obsolete brick fort, as in the case of Fort Pulaski; what he was suggesting was an artillery duel between experimental ironclads, with unsuspected weaknesses and a slow rate of fire, and well-entrenched rifled cannon. Du Pont had no faith in the expedition, and, when the ships began to suffer damage, he was quick to withdraw and end the contest. Du Pont's efforts to clear himself of blame for the fiasco led to an exchange of letters with the Navy Department and his relief from command. His querulous letters continue to be the dark shadow on an otherwise successful naval career.

Among his correspondents were Gideon Welles, G. V. Fox, Henry Winter Davis, Franklin Buchanan, Percival Drayton, John A. Dahlgren, James W. Grimes, John L. Worden, Alexander D. Bache, David G. Farragut, and John Rodgers. Most of Du Pont's letters, however, were to his wife and were full of his activities and reveal his thoughts. These letters are particularly valuable for an inside story of important naval campaigns during the Civil War.

University of Minnesota

RODNEY C. LOEHR

CONFEDERATE PROPAGANDA IN EUROPE, 1861-1865. By *Charles P. Cullop*. (Coral Gables, Fla.: University of Miami Press. 1969. Pp. 160. \$6.95.)

PROPAGANDA may never become wholly ordered and computerized; its ideological content invites too much of personal statement, and its strategies require too much of sensibility, of careful listening and intuiting on the part of the propagandist. Perhaps our increasing interest in popular psychology, and our refinement in techniques for its measurement, will end by compelling the agent of propaganda to an even defter tactile skill. But here, as in so much else, the labor of the individual in growing subtler has become less entrepreneurial and obvious than it once was. The earlier situation is represented in Charles P. Cullop's Confederate propagandists, who go to Europe on personal missions,

set up shop, make contacts, feel out moods and possibilities, and write argumentatively in some combination of strategic judgment and private political conviction.

Such was distinctively the career in Great Britain of Henry Hotze, whom Cullop joins other historians in praising for perception and general ability. Hotze ventured in many directions, guiding British sympathizers, catching upon anything useful from the cotton crisis to the admiring grief among the British at the death of Stonewall Jackson, and writing boldly about the peculiar institution to which Hotze was philosophically attached and for which he would not apologize. He was the essayist with a point of view: his *Index* criticized sympathizers with the South who were timid about slavery, and, when it appeared that the Confederacy might arm some of its black population, the paper discussed the proposal in an evaluative, commentary way, cautious but not unfavorable. Hotze and the other subjects of Cullop's study fix us at a point of history, a time when governments at war had begun systematically to address foreign publics, to consider the popular context of diplomacy—as revolutionary France had addressed the young American nation many decades before—but were not yet bringing to that enterprise the closeness and organization that the more advanced technologies of the twentieth century would attain.

University of Massachusetts, Amherst

JOSEPH M. HERNON, JR.

RECONSTRUCTING THE UNION: THEORY AND POLICY DURING THE CIVIL WAR. By *Herman Belz*. [The Beveridge Memorial Fund.] (Ithaca, N. Y.: Cornell University Press for the American Historical Association. 1969. Pp. ix, 336. \$8.50.)

LOSING THE PEACE: GEORGIA REPUBLICANS AND RECONSTRUCTION, 1865-1871. By *Elizabeth Studley Nathans*. (Baton Rouge: Louisiana State University Press. 1968. Pp. xi, 268. \$8.00.)

RECONSTRUCTION: THE ENDING OF THE CIVIL WAR. By *Avery Craven*. (New York: Holt, Rinehart and Winston. 1969. Pp. vi, 330. Cloth \$6.95, paper \$3.95.)

THESE studies of Reconstruction were produced by two young historians born in the North but teaching in the South and a veteran scholar born in the South who has taught most of his life in the North. The monographs by Belz and Nathans advance a "consensus" interpretation, in which there was little difference between Lincoln and the radicals or between the objectives of important Republicans and conservatives in Georgia, moderates rather than radicals controlled the levers of power, and conflict or failure resulted not from fundamental differences in viewpoints but from faltering leadership or bad tactics. Craven, on the other hand, adheres to a "conflict" interpretation of the Civil War-Reconstruction: it was a clash between two civilizations; the egalitarian thrust of northern radicalism coupled with aggressive, unfettered capitalism crushed the backward-looking South and sought control of the new age ushered in by northern victory. Moderates like the able Lincoln and the inept Johnson were squeezed or destroyed by the contending forces. In the end the "liberty" of laissez-faire capitalism triumphed, but the "equality" of the Fourteenth and Fifteenth Amendments was aborted.

Individually each of these volumes has interest and merit; collectively they are somewhat disappointing. Belz and Nathans have done intensive research in primary sources; the wealth of manuscript collections consulted by Nathans is especially impressive. Both books illuminate heretofore dark corners of their subjects. Craven's

study is a welcome addition to the continuing efforts by some of the profession's best representatives to explain the enigma of Reconstruction. Yet while one's stock of information is enlarged by these books, his understanding is only slightly increased. Some of the interpretations seem forced or artificial; all three studies recognize the central importance of the race question, but none provides new material or insight on the role of the Negro.

Most accounts of congressional Reconstruction begin with 1865; some include an analysis of the Wade-Davis Bill of 1864. Belz's volume provides the first detailed analysis of wartime congressional Reconstruction efforts. "Restoration" of the Union versus "reconstruction" emerged as an important issue when South Carolina seceded in 1860. To counter the President's theory that the states were never legally out of the Union and could return merely by laying down their arms, antislavery Republicans developed the concept of territorialization: by seceding, the Confederate states had ceased to be states and reverted to the condition of territories, from which they could be restored to statehood only by fulfilling certain conditions, including emancipation. But as Lincoln moved toward a policy of emancipation, Republican congressional leaders retreated from territorialization and based their theory of Reconstruction on the constitutional guarantee to every state of a republican form of government, a fuzzy concept that the President was willing to accept. By 1863, according to Belz, Lincoln and the Republican congressional majority had reached an accommodation, and the Radicals' response to Lincoln's Reconstruction plan in December was more approving than critical. Congressional opposition to presidential Reconstruction flared again in 1864, but this resulted more from procedural than substantive factors. Belz believes that, if Lincoln had lived, the schism between Congress and the President in 1866-1868 would not have occurred: Lincoln and Congress "had arrived at certain understandings concerning party, national power, and minimum guarantees for the freedmen from which a solution could have emerged."

This emphasis on the broad belt of agreement between Lincoln and the congressional Republicans is in line with recent historiographical trends, but Belz has carried the process of homogenization too far. He downgrades genuine Radicals to an impotent faction; Thaddeus Stevens is a secondary figure who gets one-third as much space in the index as Ira Harris or Henry L. Dawes. One example illustrates the distortion to which a consensus interpretation can lead: Belz repeatedly cites the abolitionist Boston *Commonwealth* as evidence that Radicals largely approved of Lincoln's reconstruction proposal of December 1863; yet all of his citations come from the *Commonwealth's* first two issues following Lincoln's message, when the editors had not yet thought through the full consequences of the President's program. Missing from Belz's account are any references to subsequent editorials in which the *Commonwealth* denounced Lincoln's policy as "deplorable nonsense," "thoroughly anti-republican," a "burlesque" that, if carried out, would make the war "a gigantic crime and failure."

In different and milder fashion, Nathans' study of Georgia also distorts reality. In her case, the emphasis is not on the consensus that *was* achieved but on the one that *should* have been. The fledgling Republican party of Georgia was confronted with three alternatives in 1867: it could try to create a coalition of the dispossessed by advocating equal rights for blacks and liberal economic legislation for marginal white farmers and blue-collar workers; it could attempt to attract the support of old Whigs and new businessmen; or it could appeal to northern Republicans for congressional and military assistance to retain power in the face of native white hostility. State

Republican leaders chose the first alternative in 1867, and by enacting tax and debtor relief laws actually won the support of a number of white farmers in north Georgia and white workers in the wire grass region of south Georgia (an interesting appendix shows the coefficients of correlation between regional-economic indexes and Republican strength). But, because of the intimidation of black voters and other factors, this coalition failed to provide the Republicans with a dependable majority, and so the party turned to Congress for help. By 1870 even Radical leaders in Congress were tired of the Georgia problem and refused to intervene further. In desperation, Republicans appealed to the Whig-businessman element, but by this time it was "too little, too late." Nathans thinks the Republicans made the wrong choices in 1867-1868. They should have tried for the alliance with old Whigs, which might have "united a natural majority of the state's voters . . . and which would have embarked on a program of economic and social development for the state similar to that advanced by Henry Grady in the 1880's." The failure of Reconstruction was not inevitable; it was caused by the mistaken political strategy of Republican leaders. This is a neat, concise thesis, but it is artificial and unconvincing. It assumes that Republicans had a free choice of tactics, that their choice would be governed entirely by tactical considerations without regard to principles or to pressure from constituents, and that Reconstruction could have been a political success irrespective of recalcitrant social, economic, and psychological realities. Nathans admits that an alliance with Whig-business interests would have "meant the adoption of programs and policies repugnant to the Negro and white laboring groups," and one wonders whether a "reconstruction" on these terms would deserve the name. Nathans has written a fine monograph of political history, but she has pushed her evidence too far and has neglected the socioeconomic context vital to a full understanding of the failure of Reconstruction.

Avery Craven sees the Civil War and Reconstruction as a titanic clash between the traditional, backward-looking South and the forces of economic modernization and social revolution represented by northern Republicans. It was a total war, and since Reconstruction was viewed as the consummation of wartime objectives, the framers of the postwar settlement meant it to be a total Reconstruction. There was little room for consensus. Unlike Belz, McKittrick, and other recent writers, Craven views as crucial the role played by such Radicals as Sumner and Stevens, who were spokesmen for the wave of Christian-democratic-egalitarian-utopian reform that swept America in the nineteenth century and tried to realize the American dream of equality and human perfection in the South during Reconstruction. Of course they failed, because their view of human nature was rooted in abstraction (there is even an index entry for "abstractionists"); like other revolutions, Reconstruction was followed by reaction "because realities had been ignored in a mad plunge for perfection for which poor, stumbling, bleeding mankind was as yet unprepared." But mankind *was* prepared for the new economic age achieved by the victory of northern democratic capitalism; in discussing this development Craven partly reflects the influence of Charles Beard and echoes Barrington Moore. And, following C. Vann Woodward, Craven asserts that in the 1870's the Republican party sacrificed the idealistic aims of Reconstruction to salvage the practical.

The book is rich in generalizations too numerous to be summarized here. One can only admire the flexibility of Craven's mind and his willingness to accept or at least to entertain new interpretations of the period. He has tried to be fair to the Radicals, though occasionally his old aversion to them slips through. He has accepted the revisionist interpretation of the Negro, partly assimilated new viewpoints toward

the scalawags, but balks at considering the carpetbaggers in a new light. Few of the generalizations are genuinely new, however; they are either restatements or amalgams of older and newer interpretations. Nearly three-quarters of the book treats the years before passage of the Reconstruction Acts in 1867, a period about which we already know much; the remainder of the volume, covering the 1867-1877 period about which we need to know more, is disjointed and episodic. This book stands as an important addition to the growing number of Reconstruction syntheses, but it gives us little that is new.

Princeton University

JAMES M. McPHERSON

RAIL ROUTES SOUTH: LOUISVILLE'S FIGHT FOR THE SOUTHERN MARKET, 1865-1872. By *Leonard P. Curry*. (Lexington: University of Kentucky Press. 1969. Pp. x, 150. \$5.95.)

At the end of the Civil War in 1865 Louisville was in a favorable position to reopen and hold the trade of a large part of the South. Its prime asset was the Louisville-controlled Louisville and Nashville Railroad, which had thrived on war traffic and which held a fine strategic position for the postwar trade. The dominance of river transportation was fading. Cincinnati, with its trading area primarily to the northward, had to depend on the rivers for its southern trade, except for the adjacent area in Kentucky. Cincinnati's efforts to secure through rail shipments by a rail connection at Louisville were essentially defeated when the latter city successfully insisted upon a difference in track gauge between the Cincinnati line and the connecting lines at Louisville. Evansville, too, sought to secure access by rail to the southern trade, but it was restricted by Louisville's enterprise in building the controlling railroads.

Cincinnati's great effort to reach the South was concentrated upon the construction of the Cincinnati Southern Railroad from Cincinnati to Chattanooga. Much of Professor Curry's book deals with the efforts of Cincinnati to get the approval of the Kentucky legislature for this enterprise and with the opposition of Louisville and the Louisville and Nashville Railroad Company. In 1872 the legislature gave its approval, and construction of the road got under way. It was opened in 1880.

This is a short book, and the author does very well what he set out to do. He gives us a needed building block for the construction of the story of the commercial and railroad rivalries of the South. He makes some contribution, too, to a better understanding of the local political history of Kentucky. Although Curry has done his own research, much of the story has been previously accessible in several works on the Louisville and Nashville and the Cincinnati Southern Railroads. The related ground that he does not cover is vast, but it is probably unfair to criticize a book for what it is not. Curry has added illumination to a difficult and important subject.

University of Alabama

JAMES F. DOSTER

AN AMERICAN DISSENTER: THE LIFE OF ALGIE MARTIN SIMONS, 1870-1950. By *Kent and Gretchen Kreuter*. (Lexington: University of Kentucky Press. 1969. Pp. xii, 236. \$7.50.)

In the eighty years before his death in 1950, Algie Martin Simons gradually outgrew his youthful socialist militance and returned to the Republican faith of his forebears. Although he broke with the Socialist party over American entry into the First World

War, his political and ideological migration from Left to Right was less abrupt than his departure from the party indicated.

Simons' radicalism was the indigenous American brand, however orthodox his Marxism may occasionally have appeared. At the University of Wisconsin he had heard Frederick Jackson Turner and Richard T. Ely challenge the economic and social dogmas of their contemporaries. Social work in Cincinnati and Chicago made Simons susceptible to the Christian socialism he discovered in settlement houses. He progressed to Daniel De Leon's "scientific" Socialist Labor party and then to the moderate Marxism of Berger, Hillquit, and Debs. An occasional candidate and party functionary, Simons wrote for the socialist press for about twenty years. In his vehement phase he helped found the IWW and edited the *International Socialist Review*; as he mellowed, he edited the *Coming Nation*, a polite weekly that provided cultural inspiration for socialist families. His biographers suggest that early periodical versions of *Social Forces in American History* were superior to his book published in 1911. As Simons grew more conservative after leaving the socialist movement, he turned out patriotic tracts for the Wisconsin Loyalty Legion, texts on scientific management in the 1920's, and pamphlets opposing health insurance for the American Medical Association. He thought Henry Ford an attractive presidential possibility, voted for Herbert Hoover, and opposed much of the New Deal.

Simons lacked, Kent and Gretchen Kreuter believe, a reliable intellectual rudder. From his boyhood in Wisconsin through his service to the AMA, he was a debater who accepted a proposition and then looked up arguments. His future in the socialist movement was never more bleak than when he left it; pride, as well as patriotism, contributed to that decision. His nationalism, however, was constant and is demonstrated in his concern for the nation's history and his attempt to give radicalism a distinctly American slant, as well as in his position in 1917.

The Kreuters have used extensively the Simons collection at the Wisconsin State Historical Society. They have also sampled other manuscripts, read the Socialist press, and relied on previous unpublished studies of Simons. Their interpretation is uncomplicated and convincing, their prose superior to that of Simons himself. He will not need another biography.

Phillips Exeter Academy

HENRY F. BEDFORD

TOWARD COMMON GROUND: THE STORY OF THE ETHICAL SOCIETIES IN THE UNITED STATES. By *Howard B. Radest*. [Ethical Culture Publications. Published in collaboration with the American Ethical Union.] (New York: Frederick Ungar Publishing Company. 1969. Pp. xii, 348. \$8.50.)

THE dust jacket reads: "A history of Ethical Culture is a history of the times." This is a reasonable assertion, but one the volume fails to demonstrate. Mr. Radest has little to say about ethical culture's relationship, direct or casual, to such movements and moods as modernism, scientism, positive thinking, mental health, *noblesse oblige* and patrician reform, status anxiety, social engineering, the Americanization of Jews, the contending merits of exhortation and force in effecting social change, or the "death of God" and "secular city" debates. This is a pity, for ethical culture, it seems to me, touches on, and therefore should illuminate, all of these manifestations "of the times."

The study is one of missed opportunities because of narrow research as well as of narrow conceptualization. No manuscript materials and almost no primary sources of

any sort were examined other than those of the movement itself, and this deficiency is not redressed by a wide reading in secondary works. Save for Goldman's *Rendezvous with Destiny* and a couple of other books, the author seems unaware of the vast monographic and periodical literature dealing with the varieties of religious and reform experience in modern America. The eccentricity of the bibliography is suggested by the inclusion of fourteen volumes by David S. Muzzey, mostly history textbooks; Muzzey just happened to be an adherent of ethical culture.

The dust jacket reads: "This is an official history of Ethical Culture from its founding in 1876 up to its Seventy-fifth Anniversary." Radest honors this assignment in workmanlike fashion. He successfully tells a little-known story and tells it with surprising objectivity. I say "surprising" because Radest is executive director of the American Ethical Union, the federation of Societies and Fellowships for Ethical Culture, and also because of such chapter titles as "Ecce Homo: Felix Adler" and because the research assistant conducted all interviews and oral histories with "kindness and perceptiveness" in order to "recapture precious memories for the record." The portrait of Adler is honest (and slightly unattractive); the portrait of John Lovejoy Elliott is honest (and engaging); the tensions splintering the movement are frankly faced; and the failure to enlist more than a few thousand followers is manfully admitted. (Eliminate New Yorkers from the rolls, and the number is reduced to hundreds.)

Much in ethical culture is admirable: its compassion for the underdog, its moral responsibility, its awareness that we rarely sin against God alone (Rauschenbusch's words), its Emersonian concern to free the individual as well as society. And its reasonableness, rationality, and genuine decency seem especially precious qualities if only because of their rarity today. The obverse side of the coin is its exclusiveness, its abstractness, its coolness, its innocence, and (as it would seem to most of us) its inability to sustain men as they confront the mystery, contingency, and terror of life—and death.

Henry Sloane Coffin once observed that too many churchmen were bent on becoming ranchers rather than shepherds. One of the pleasant things about ethical culture is that it did not much go in for proselyting, and its worth may not be measured by its numbers. Perhaps it is just as well, for it would require a leap of faith to believe that this volume would warm and win many hearts.

University of North Carolina, Chapel Hill

ROBERT MOATS MILLER

FROM HAYES TO McKINLEY: NATIONAL PARTY POLITICS, 1877-1896. By H. Wayne Morgan. ([Syracuse, N. Y.:] Syracuse University Press. 1969. Pp. x, 618. \$12.95.)

"This book has two major purposes," H. Wayne Morgan explains in his preface, "(1) to detail and unify the events of national politics between 1877 and 1896, the years that produced a working national party system; and (2) to show how parties differed and how each met the major issues of the time." This is "a view of politics *at* the top, and *from* the top" (p. vi). But, in a larger sense, it is an attempt to redeem the political history of the period from its detractors. They found its issues to be almost insignificant, its politicians to be humbugs, and its political parties—perhaps the Populists excepted—as similar as Tweedledum and Tweedledee. Morgan writes to set the record straight. Politicians, both major and minor, who appear on the national scene, are grist for his mill, and he endlessly grinds out

colorful vignettes of each of them. National elections in the period and some state contests are subjected to the same treatment. And, because this is a big book, Morgan has space to describe the physical, emotional, and occasionally the intellectual setting. The scholar in search of clever quotations and witty observations will find that Morgan has rifled scores of biographies and manuscript collections to provide his readers with full bounty. In this sense the book is a tour de force. But many readers will remain unconvinced by Morgan's evidence that there were *real* differences between Republicans and Democrats.

Morgan is aware of the shortcomings of his approach. He recognizes that he cannot make his thesis explicit, and he hopes that through "cumulative, analytical narrative" his viewpoint will emerge. It does, but all too frequently detail seems to have been substituted for analysis. As a result, scholars familiar with the period may feel they are reading an enormous chapter in a textbook or a long brief written by an informed, clever, nineteenth-century Republican lawyer. The structure is traditional. The rhetoric is conventional. Detail is heaped upon detail, but the end product is conventional wisdom. Except for Morgan's strong advocacy of the Republican party's position on money, the tariff, nationalism, and expansion—all of which he has made clear in his earlier work—there is little that is new.

But Morgan writes extremely well. His characters stand out clearly, and their drive for power is evident. His narrative is rarely ponderous. He achieves much of his literary success through the use of similes, some of which are eye catching: "Cabinet-making was as elaborately political as in any European state, colorful and intricate like the peacock's mating dance" (p. 121). Despite his usual success in capturing the essence of a scene, there are occasional gaffes. For example, the reader is somewhat surprised to learn that Hayes's mother, rather than the candidate, took an oath at his inaugural: "He [Hayes] sat for a moment in one of Washington's chairs, and saw his mother survey the crowd before rising to take the oath of office" (p. 127).

There is an imbalance in the book. Morgan analyzes the causes of agrarian discontent, but the tariff, money, railroad regulation, and many other issues are dealt with primarily in terms of their appeals and effects on the political parties. He also tries to keep his narrative uncluttered, but he inserts gratuitous observations as footnotes (his documentation—sixty pages of it—is in endnotes), and these lack the cadence, tone, and temper of the text. The imbalance may be a product of Morgan's method of presentation. For example, although he repeatedly asserts that significant changes were taking place in the intraparty structure, he never finds time to analyze the evolution of the national committees because he is too involved in describing the personality and behavior of the more colorful members of the committees. Moreover, Morgan insists that the Republican party, unlike the Democratic party, became a national institution because its strong presidential candidates focused on issues and suppressed and controlled the local or regionally oriented bosses; but, because personality clashes are so important in Morgan's narrative, his explanation of the election of 1896 discloses McKinley still fighting local bossism (Platt and Quay). Morgan is, of course, right about the evolution of national parties, but his stress upon the role of personalities in the process weakens rather than enhances his case.

Morgan's research is basically sound. His bibliographical note attests to familiarity with the literature and many manuscript collections. He also points out topics in need of reassessment, and his evaluation of the existing literature will probably prompt some controversy.

If, as some quantification-oriented historians assert, the best way to measure the value of a book is to do a content analysis of its index, Morgan's study will fare badly. The index, only five pages in length, is nonanalytical and consists primarily of the names of people, laws, and events. Ideas are absent. Most readers will be more generous in their assessment of the book.

Indiana University

MARTIN RIDGE

THE PAPERS OF WOODROW WILSON. Volume V, 1885-1888; Volume VI, 1888-1890. *Arthur S. Link et al.*, Editors. [Sponsored by the Woodrow Wilson Foundation and Princeton University.] (Princeton, N.J.: Princeton University Press. 1968; 1969. Pp. xv, 792; xii, 733. \$15.00 each.)

THESE volumes cover Wilson's years as a member of the faculties of Bryn Mawr College and Johns Hopkins and Wesleyan Universities. The correspondence clearly reveals Wilson's development as scholar and teacher, his increasing stature in the academic profession, and, finally, his appointment as a professor at Princeton. During these years, Wilson became a pioneer in the study of public administration, writing *The State*, a study of the development of democracy in the modern world, and undertaking a historical venture, a book later published under the title of *Division and Reunion*.

From the correspondence, one gains considerable insight into a variety of subjects: the tensions between faculties and the administrators of colleges and universities; the problems encountered by one who attempted to be a pioneer in a new discipline; the relations between scholars and publishing companies; the process by which college and university appointments were made; the existence of a tightly knit fraternity of scholars within the historical profession.

Even though the correspondence reveals much about the inner quality of Wilson, there is very little hint in these volumes that Wilson had any desire to become an educational or political leader, or that he would emerge as such. His life was devoted to scholarship, and while he had a strong interest in contemporary politics, it was the interest of the detached observer. The volumes also contain a vast correspondence between Wilson and his wife. Though these familial letters reveal much about Wilson's intimate life, they are rather thin in social content and tell us very little about the institutional fabric within which he lived.

In view of the present interest in comparative politics, it is indeed stimulating to read Wilson's papers, for he was an important pioneer in the study of comparative public administration, moving in some of the directions taken by Talcott Parsons and Gabriel Almond a half century later. Indeed, had Wilson not left the field of scholarship to become president of Princeton, the field of comparative politics might have become important earlier than it did in America.

These volumes, like their predecessors, represent an outstanding editorial achievement. The letters are extremely well annotated, a result of what may be the most meticulous research project dealing with American history over the last eighty years. There are lengthy editorial notes scattered throughout the volumes, with the result that the papers are placed within a broad historical context. The index, excellently arranged, permits ready reference use of the volumes. The editors are to be commended for deciphering the scratchy handwriting of dozens of individuals whose correspondence might otherwise have remained useless, and they especially are to be praised for the transcription of Wilson's shorthand.

At the same time, one wonders if the editors have not been excessive in their inclusion of materials. Clearly, what to include and what to exclude constitute delicate matters of judgment. But I question the utility of including fragmentary classroom lectures, which often have little meaning in themselves; Wilson's marginal notes in books; titles of books and articles that Wilson gathered, but may never have read; and portions of books and articles readily available elsewhere.

Despite these quibbles, scholars are indeed fortunate to have these volumes. And if our society has seen fit to finance the publication of dozens of volumes of papers of one American, perhaps it will now provide even larger sums for the collecting of data about the masses of Americans—data that can easily be disseminated to scholars throughout the world in a form that can be read through the use of a machine. Only when we have a much fuller knowledge of the interaction of elites such as Woodrow Wilson with different levels of society will we significantly alter our understanding of American history. These volumes represent a great achievement in improving our understanding of American history at the elite level; it would be well if we could soon make equally significant progress in understanding our society at other levels.

University of Wisconsin, Madison

J. ROGERS HOLLINGSWORTH

PRAIRIE FARMER AND WLS: THE BURRIDGE D. BUTLER YEARS. By James F. Evans. (Urbana: University of Illinois Press. 1969. Pp. 329. \$8.50.)

ON July 15, 1909, BurrIDGE D. Butler bought the faltering *Prairie Farmer*, Chicago's oldest farm newspaper. Butler, with a vision of a rural weekly "just as keen as the city daily," succeeded by 1915 in making his property the leading agricultural newspaper in the Middle West. In 1928 Butler also became a pioneer of Chicago radio when he added fledgling station WLS to his holdings.

James Evans' thorough biography, a first, explains the techniques Butler used to achieve his success. A firm believer in the agrarian myth about the inherent superiority of rural over urban life, Butler produced his newspaper for the edification and uplifting of the "just plain folks" who peopled the farms of downstate Illinois. The same philosophy guided WLS in the 1930's and 1940's and made it the leading station for rural mid-America.

Butler learned the newspaper business with the Scripps-McRae League in the late 1890's, when E. W. Scripps was building his empire. Although limited in writing proficiency, Butler had a genius for editorial organization and an ability to seek and find the journalistic and broadcasting talent that could bring his message to rural America both effectively and profitably.

A man of unsophisticated tastes and habits, Butler stressed material that city people would consider "corny." He referred to farmers as "my family," and promoted their interests with the concern of a bucolic patriarch. Since he considered smoking and drinking immoral, his properties refused all advertising for cigarettes and alcoholic beverages. Editorial debate once raged over whether farm people should play baseball on Sunday. Also, under Butler's direction, *Prairie Farmer* and WLS promoted cornhusking to the number one sport in rural America by 1941.

In the 1930's, the "National Barn Dance," which specialized in Butler's particular brand of country humor, became a Saturday night ritual in millions of homes. Fibber McGee and Molly, George Gobel, Gene Autry, and Lula Belle and Scotty were among the stars the show produced. Female performers on the "Barn Dance" were forbidden

to wear skirts two or three inches above the knee because Butler felt his people "didn't drive hundreds of miles to see a girlie show."

Evans' study is a useful addition to the history of midwestern journalism. Agricultural historians will find the chapters dealing with the work of Clifford V. Gregory, Butler's editor from 1914 until 1936, invaluable in explaining why the farm belt supported the New Deal and Henry A. Wallace. Another contribution of this study is its thorough history of WLS and early Chicago radio.

Wisconsin State University, Oshkosh

JUSTIN E. WALSH

HENRY JAMES: THE TREACHEROUS YEARS, 1895-1901. By *Leon Edel*. (Philadelphia: J. B. Lippincott Company. 1969. Pp. 381. \$10.00.)

HENRY James once wrote of a definitive biography of George Sand that it seemed only "a tub of soiled linen which the muse of history, rolling her sleeves well up, has not even yet quite ceased energetically and publicly to wash." Recent biographies of Sinclair Lewis, Eugene O'Neill, and Ernest Hemingway suggest that the muse of history is still at her thorough work, but Leon Edel's biography of James—now in its fourth of five volumes—cannot be so described. Although its basic scholarship will stand comparison with any of these others, Edel has invoked the muse of psychology to interpret his data and thus presents a single and completely clothed man. Whether it is the real Henry James or not will depend upon the faith that one places in the biographer's basic thesis.

The thesis upon which this volume is constructed, although a part of the theory underlying the work as a whole, has within itself all the form and clarity of the plot of a well-planned novel. Edel has saved for this volume the history of James's unfortunate attempt to succeed as a playwright in order to provide the background for the traumatic experience of the failure of *Guy Domville* in 1895. When cries of "author" tempted him to appear on the stage at the curtain of the opening night, which he had carefully avoided attending, only to be greeted by the hisses and boos of the gallery, his ego received a nearly mortal wound. During the next five years he returned to fiction and "showed man's capacity to heal himself by a retreat to earlier experience."

One result of this process was the emergence of a new literary form with *The Spoils of Poynton*, a short story expanded into a novelette by the use of the scenes, dialogue, and action of a drama. Although James also wrote short short stories during these six years (up to *The Sacred Fount* in 1901), this was his predominant form, and it provided him with the techniques of verbal elaboration and objective presentation that distinguish his later and longer novels from those of the period prior to 1890.

But to Edel a more important result was the use by James of children to relive in fiction the emotional crises and stages of development of his childhood and youth and so find his way back to his full vocation as a major literary creator. At this point, psychiatry (no longer mere psychology) takes over, and the average reader may not go the whole way with Edel in seeing a succession of young girls—from early childhood through adolescence—living perceptively in a morally corrupt world but retaining their immunity with the aid of the uninvolved wisdom of innocence, as the alter ego of James himself trying to relive the problems of his formative years and so discover the right road which somewhere he had missed. If one can accept his premises and his method, Edel's interpretation is magnificently enlightening and convincing. Cooler reason suggests, however, that although this interpretation may be the truth and nothing but the truth, it surely is not the whole truth.

With only the final volume ahead, and in it the study of the "major phase," which begins here with the undertaking of *The Ambassadors*, we can be confident that we will have at the last a masterpiece in its genre—the modern psychological biography of a major literary figure. It has been a lifework for Edel who, with clear singleness of aim and exhaustive study, has demonstrated what psychology—and psychology alone—can contribute to the understanding of literary history.

University of Pennsylvania

ROBERT E. SPILLER

WILLIAM JENNINGS BRYAN. Volume II, PROGRESSIVE POLITICIAN AND MORAL STATESMAN, 1909–1915. By *Paolo E. Coletta*. (Lincoln: University of Nebraska Press. 1969. Pp. viii, 380. \$8.95.)

FIVE years ago Coletta promised *Political Puritan*, a concluding volume to his biography of the Great Commoner. The present installment, therefore, comes as a pleasant surprise for, while announcing a third volume with that title, it offers a detailed treatment of the politician's middle years—and all to the good. After looking into Bryan's continued preaching of Progressivism following the defeat of 1908 and his subsequent switch from antiprohibitionist to prohibitionist, Coletta assays his role in the Democratic National Convention of 1912. Despite the work, notably, of Link, he judges Bryan's part during the Baltimore proceedings sufficiently important to term him "Wilson's Warwick." Certainly he makes a good, if not an absolutely convincing, case. The remainder of the book, some three-quarters of it, is given to Bryan's service in the Wilson administration. It examines the eager dispensing of patronage that caused him to become known as "The Prince of Job Hunters"; weighs his influence with the House and Senate in helping greatly the cause of New Freedom legislation, especially on banking; and gives in full measure the handling of foreign affairs respecting Mexico, the Far East, world peace, and the crucial problem of neutrality that brought Bryan to resign for reasons of conscience rather than sign the second *Lusitania* note. The agony that decision cost him is particularly well described.

Despite his pronounced sympathy for Bryan, the author does not hesitate to criticize. He has hard words for the junketing on the Chautauqua circuit when there were pressing, even emergency, matters of foreign relations; for Bryan's blunt statement that he had to lecture (he "Chautalked") since his official salary was inadequate to meet his living expenses; for his saying that he might be a candidate in 1916, a remark hardly calculated to impress Wilson favorably. Moreover, he holds Bryan, with minor exceptions, no great Secretary of State, agreeing with others that he sought solutions to complicated diplomatic problems in simple morality. As in his previous volume, Coletta expresses considered opinions. He defends Bryan from the charge that he demanded an American arms embargo in 1914; he is satisfied that there was an informal understanding between Great Britain and the United States on British support for American policy in Mexico in exchange for the repeal of exemptions from Panama Canal tolls of American vessels; he sees the influence of Bryan's peace plan in the League of Nations' Covenant, the Kellogg-Briand Pact, and the fact-finding commissions of today's United Nations; he thinks Bryan might well have been awarded the Nobel Peace Prize. Still, there are places—for example, the narration of Page's role in the *Dacia* seizure—where he makes too little of graphic tidbits. The explanation of the Hitchcock-Bryan squabble is marred by the printer's nodding (p. 102). In addition, detractors of Bryan will find that Eldon Penrose, whose article is cited, perceives more in the Secretary's California trip of 1913, and

that Ray Ginger, in his revealing Bryan anthology of 1967, furnishes further information concerning Bryan's views on racial segregation in Washington. Bryan continues to stir pens. Every student acquainted with Lawrence Levine's stimulating *Defender of the Faith* (1965) awaits Coletta's last chapters and summation of Bryan's career.

City College of New York

JOSEPH A. BOROMÉ

PRESIDENT WILSON FIGHTS HIS WAR: WORLD WAR I AND THE AMERICAN INTERVENTION. By *Harvey A. DeWeerd*. [The Wars of the United States.] (New York: Macmillan Company. 1968. Pp. xxi, 457. \$12.50.)

THE WAR TO END ALL WARS: THE AMERICAN MILITARY EXPERIENCE IN WORLD WAR I. By *Edward M. Coffman*. (New York: Oxford University Press. 1968. Pp. xvi, 412. \$9.75.)

ALTHOUGH these volumes overlap in places, they differ in purpose, emphasis, and coverage. Both rest on sound scholarship. Both provide a useful overview of a field that United States historians tend to neglect. In many conclusions, the authors agree. In each case, the contents are best described by the subtitle.

Writing in Louis Morton's new series, DeWeerd concentrates on Europe. He seeks to place the role of the United States in proper perspective, insisting that to present the European phase of the war as a background to the American effort leads to distortion. Accordingly, he devotes half of his text to the period before the United States entered the conflict. In only the first of his initial seven chapters—"America's Road to War, 1914-1917"—does the United States occupy the center of the stage. The other six trace European diplomatic developments from 1871 to 1914 and describe at length the campaigns of 1914, 1915, 1916, and 1917. Then come three chapters on aspects of America's war program, on the controversies it engendered, and on selected domestic problems. Returning to the western front, the author allots four of the remaining nine chapters to the German offensives of 1918, one to Pershing's insistence on maintaining a separate United States army, two to the American drive at St. Mihiel and in the Meuse-Argonne, one to the advance culminating in the armistice, and one to a summary of the American contribution to victory.

DeWeerd, then, emphasizes the strategy and tactics of coalition warfare, as seen from the headquarters of the Allies and of the American Expeditionary Force. Despite the title, his readers view the war through the eyes of John J. Pershing, not Woodrow Wilson. The index has more entries for Erich Ludendorff than for the American President, more for Sir Douglas Haig than for Newton D. Baker. The author virtually ignores the navy, the aviators, and the supply services. He shows little interest in the enlisted man. For him, military history is battlefield operations at the highest level.

Coffman conceives of military history more broadly. He purports to cover the planning, organization, and administration of the American war effort, as well as the fighting, and in terms of the men who made the decisions and executed them. Yet, even for him, the military experience of 1917-1918 is not a total one. He does not attempt to view the period from the White House, Congress, or the State Department; he intentionally excludes those at home who staunchly supported or strongly opposed the "Great Crusade." His focus is on the American armed forces, not on the Allied or enemy forces. He pays particular attention to the enlisted man and junior officer, and he successfully exploits the records and reminiscences of those far down the military

ladder. He covers events before April 6, 1917, in one short chapter and allots only three of the remaining ten chapters to operations on the western front. The other seven discuss the War Department in 1917, the expansion of the army in the United States, the molding of the AEF in France, the conflict between the general staff in Washington and general headquarters in Chaumont, the victory at sea, "The Romance and Reality of the Air War," and the early problems of peace and demobilization.

Both interests and sources lead Coffman to topics missing in DeWeerd. These include the operation of the draft, the procurement of officers, the performance of Negro troops, the treatment of conscientious objectors, the fostering of morale, the control of disease, the adequacy of weapons, and the rivalry between regulars, conscripts, and militia. Coffman does not neglect grand strategy, but he spends less time than DeWeerd in analyzing the problems of the other belligerents and more in considering those of the AEF at the corps and division level. He has a keen eye for detail. His figures come alive. In a few words he tells what relatively obscure soldiers of 1917-1918 achieved in 1941-1945. Such well-known scholars as William L. Langer, Chester V. Easum, and Frederick A. Pottle appear briefly but appropriately in his pages.

Both volumes are handsomely printed, but DeWeerd's has the edge in the number and value of the maps. He offers fifty-two of assorted sizes plus two sets of end papers; Coffman musters only seven crowded ones with identical end pieces. DeWeerd's illustrations are also superior in quality, but where all of his depict statesmen, monarchs, generals, and admirals, Coffman's include enlisted men, air aces, submarine chaser skippers, training camp scenes, gas mask drill, work details, and trench life. DeWeerd's fourteen-page "selected bibliography" lacks annotation, but it is supplemented by a short note on the official histories of the war and a list of manuscript collections examined. Coffman's very valuable "essay on the sources" runs to thirty-two pages and describes a much more extensive array of unprinted materials, as well as interviews, many of which were used for the biography of Peyton C. March published in 1966. The DeWeerd volume has both explanatory footnotes and citations at the end of each chapter; Coffman's book is not documented. His index, however, is excellent, while DeWeerd's is grossly inadequate. It contains almost no subject entries, and there are no divisions within the name entries.

If neither author offers major new interpretations, each reaches sound, balanced, and often similar judgments. They agree that the American military effort was decisive in defeating Germany. Both concede that this effort depended upon weapons, supplies, transport, and training provided by the Allies. Both stress deficiencies in the American endeavor. But, as DeWeerd warns, the United States' accomplishments can be only partly quantified; much of its contribution was in the intangible sphere of influence and morale. Baker and Pershing show to advantage in these pages, though the faults of each are noted. The latter's rejection of an understandable Anglo-French desire to amalgamate fresh American troops with veteran Allied divisions is deemed wise. Ironically, in view of the American tradition of civilian supremacy and of Wilson's innate distrust of the military, Pershing wielded powers in 1918, as Coffman points out, that neither Clemenceau nor Lloyd George granted to their generals. In both books, Wilson is a rather enigmatic figure. DeWeerd's title is misleading, for his materials show that it was American intervention, not the President's direction of the war, that provided the key to the Allied victory. As for Coffman, he does not even mention his title until the very last sentence of the book.

Northwestern University

RICHARD W. LEOPOLD

MR. JUSTICE MURPHY: A POLITICAL BIOGRAPHY. By J. Woodford Howard, Jr. (Princeton, N.J.: Princeton University Press. 1968. Pp. x, 578. \$12.50.)

IN the conventional estimate, Frank Murphy was an archpractitioner of New Deal politics, but a fish out of water on the highest court. We now have a lengthy biography—it is called a “political” biography explicitly to exclude strictly private matters and implicitly to make a point about the judicial process—that compels us to revise that estimate. The first half of the book, which recounts the rise of Murphy as a public figure, shows how well that ascent was served by the attunement of his political personality—his activist temper, his moralistic rhetoric, his deep compassion for the underprivileged, his large capacity for self-promotion—to the needs and opportunities of depression times. And the second half of the book, which tells the story of Mr. Justice Murphy, does show that the exegetical canons were sometimes violated by his sacrifice of tradition for result. But the picture that emerges from these pages is neither that of a prototypical politician nor that of a crude and opportunistic jurist. There was nothing commonplace about the fact that an Irish Catholic in the 1920’s became as devoted to civil rights and civil liberties as to social welfare, or that a mayor supported by Right-wing elements fought to squeeze large relief funds from a parched economy, or that a governor confronted by illegal sit-downs refrained from calling out the police. The explanations offered for these anomalies—his anticlerical Catholicism, his preference for personal rather than machine-based politics, his fear of governmental force as a social arbiter—place him in a far from common mold. Contrarily, in deciding cases, Murphy often yielded to the demands of institutional loyalty and the constraints of legal craft. When he compromised his libertarianism, as he did in the Japanese relocation cases, he did so in order to help “mass” the Court, not to honor outside obligations (his dissents on war power make it clear that he was in no sense a lackey of Roosevelt). When he could muster a majority for his libertarianism, as he did in *Schneiderman v. US*, he would tether his principles to a careful ruling. When he stood alone, or with Rutledge, in libertarian dissent, as he did in *Adamson v. California*, he offered the concepts, if not the prose, that would appeal to the Court in a later day. Drawing on intra-Court memorandums to reconstruct the interplay of Murphy with his peers, the author, through tireless explication, fits him fully into that testy collegiality known as the Roosevelt Court.

Columbia University

WALTER P. METZGER

HARRY ELMER BARNES, LEARNED CRUSADER: THE NEW HISTORY IN ACTION. Edited by Arthur Goddard. (Colorado Springs: Ralph Myles, Publisher. 1968. Pp. lxxxvi, 884. \$10.00.)

DESCRIBED by Carl L. Becker as “the learned crusader,” Barnes was a scholar and journalist noted for his learning and advocacy and sometimes praised for his scholarship. He was the most vigorous publicist of the New History, the leading campaigner on behalf of World War I revisionism, the spiritual father of World War II revisionism, and a curiously confused opponent of the cold war. In his long career he also contributed to criminology, the study of social problems, and the history of social thought and sociology. As a sociologist, and more frequently as a historian, he engaged in fierce battles. Abrasive and polemical, he was a tenacious warrior who sought to bludgeon his adversaries into submission. But, by the early 1950’s, criticism of his historical scholarship, undoubtedly reinforced by hatred of his attacks on Roosevelt’s foreign policy, closed all historical journals to him.

His career, achievements, and crusades offer rich possibilities for biography and intellectual history, for an analysis of the later revisionist movements or an assessment of the New History and the early efforts at relating sociology and history. A *Festschrift* dedicated to an analysis and appraisal of Barnes could be rewarding. But this *Festschrift* is a profound disappointment; it is a tedious and repetitious collection of frequently rambling essays.

"In the field of testimonial volumes, there is nothing quite like [this one]," announces the blurb on the jacket. The editor, a former English teacher and now an examiner for the New York City Board of Education, accepted a few analytical but contentious essays and many affectionate reminiscences and partisan tributes. Much of the book is characterized by such essays as Frederic Stone's ". . . Barnes' Outdoor Life as a Bucolic Sportsman and Frustrated Athlete," Willoughby Waterman's ". . . Personal Reminiscences," George Lundberg's discussion of a squabble with W. Stull Holt, testimonials by Barnes's former students, and reports of his assistance to students. Some contributors, however, like Clifford Kirkpatrick, Harold Faulkner, and Merle Curti, refuse to subscribe to the general theme of extravagant praise and seem dutiful but reluctant and uneasy in this collection; their essays skirt relevant subjects in what may be an effort to avoid sharp criticism of Barnes.

For historians, four essays are likely to be useful, albeit partisan and often tendentious: Margaret Fisher's pedestrian summary of Barnes's career; William Neumann's sympathetic discussion of Barnes as a "World War I Revisionist," Henry Adams' vigorous defense of him as a "World War II Revisionist"; and Murray Rothbard's enthusiastic analysis of Barnes as a "Revisionist of the Cold War." Each endorses the same general conclusion: Barnes's quest for the "truth" enraged liberal scholars who finally and unfairly drove him and those who shared his ideas out of the professional journals and away from well-known publishers. Unfortunately, Adams, in taking up the cudgels for Barnes, weakens his analysis by failing to distinguish between the profession's responses to different revisionist charges: that the Roosevelt administration's refusal of a reasonable compromise with Japan made war inevitable; that the President understood that this refusal made war inevitable and that by late 1941 he wanted war; that he had long slyly followed policies that he believed would lead to a war that was necessary to protect America; that he wanted the Japanese to attack Pearl Harbor in order to catapult a reluctant American citizenry into war. Rothbard examines the shifts in revisionism on war and seeks to explain why earlier revisionists divided on the cold war. In addition, he summarizes Barnes's strictures against contemporary court historians and the "defense intellectuals" and his warnings against the false fear of Soviet aggression against the West and the emergence of an Orwellian world at home. Unfortunately, Rothbard relies upon some dubious sources and overlooks many critics of the cold war, thereby rendering his analysis fragmentary. These last two essays, despite serious defects, raise important questions about the historical profession in the past three decades and its treatment of those who clashed with the prevailing faith. But, tucked away in this ill-conceived volume, these questions are likely to be disregarded.

Stanford University

BARTON J. BERNSTEIN

NELSON T. JOHNSON AND AMERICAN POLICY TOWARD CHINA, 1925-1941. By *Russell D. Buhite*. (East Lansing: Michigan State University Press. 1968. Pp. 163. \$6.00.)

NELSON Trusler Johnson spent nearly thirty years as a diplomat in China, serving as American minister and eventually ambassador between 1929 and 1941. Joseph C. Grew, his counterpart in Tokyo during the turbulence of the thirties, has earned a gifted biographer. Surely Johnson deserves equal time.

Professor Buhite's slim volume makes extensive use of the Johnson Papers in the Library of Congress; he has had assistance from the Johnson family and at least one of the ambassador's close colleagues, the late Stanley K. Hornbeck. He relies as well on Dorothy Borg's exhaustive studies of the late twenties and mid-thirties. The result should have been a full portrait of the man and a sophisticated probing of the extraordinarily complex issues that engulfed him.

What emerges instead is generally one dimensional on the man and uneven on the issues, a product perhaps of premature publication. The approach to Johnson is often uncritical, sometimes heavily eulogistic. "Always he was dedicated, patriotic, and highly competent," we are told. It is repeatedly argued, without supporting evidence, that Johnson's views "were undoubtedly a factor" in one major Washington decision after another. So intense is the author's loyalty to his subject that Borg is gently chided for not emphasizing "the important role played by Johnson," as indeed are Messrs. Grew and Hull for neglecting Johnson in their memoirs.

As for the issues, Buhite alternates between giving the reader too little and too much. On the one hand, Chinese history is capsulized into near meaninglessness; on the other hand, the treatment of the diplomacy of extraterritoriality and silver is dryly and lengthily familiar. One difficulty may stem from the dual title. The author sets out to deal with two subjects, and he moves uneasily from one to the other, using Johnson as a makeshift bridge. Another difficulty may be reflected in the bibliography where Waldo Heinrichs on Grew, Akira Iriye on the twenties, and James Crowley on Japanese policy in the thirties are conspicuously absent.

Johnson was neither the central figure this book makes of him, nor was he merely a cipher. Johnson's likably homespun cast of mind, his sentimentality and exasperation about the Chinese, his endless and sometimes tedious philosophizing about his fellow countrymen—all make him a prototypical American innocent in a convulsive and tragic era that needs much more critical scrutiny.

Harvard University

JAMES C. THOMSON, JR.

DEMAGOGUES IN THE DEPRESSION: AMERICAN RADICALS AND THE UNION PARTY, 1932-1936. By *David H. Bennett*. (New Brunswick, N.J.: Rutgers University Press. 1969. Pp. x, 341. \$10.00.)

Is money the root of all evil or the solution of all problems? These propositions are not mutually exclusive. In a middle-class, money-oriented society it is possible to believe in both at the same time. Professor Bennett's "demagogues in the depression"—Father Coughlin, Dr. Townsend, the Reverend Gerald L. K. Smith, and Representative Lemke—are cases in point. All of them attributed the nation's woes to unwise and improper monetary and fiscal policies and believed that reform and redirection of these policies would put the country on the road to prosperity and social justice.

In a prologue Bennett offers glimpses of the conventions of the Townsend clubs and Coughlin's National Union for Social Justice, which met in Cleveland in the

summer of 1936. Both meetings were intended to advance the presidential candidacy of Lemke and his newly organized Union party. The latter meeting ended abruptly and on an unfinished note when Coughlin collapsed in the middle of the final speech. As it turned out, Coughlin's collapse was prophetic of the fate of the Union party.

The first part of the volume traces the careers of Coughlin, Lemke, Smith, and Townsend, and to some extent their followers, to 1936. The author presents a fairly sympathetic picture of Lemke; he treats Coughlin and Townsend at greater length and with more critical analysis. Smith, "insincere and unprincipled in almost all the adventures undertaken in his short but spectacular career," and driven by intense hatred of Roosevelt, emerges as the linchpin of the coalition. The second part deals with the formation of the Union party and traces the electoral campaign in remorseless detail. Nevertheless, the sheer weight of detail concerning the organization and financing of the campaign and the vexed relations between the leaders enlarges our understanding and knowledge of an oft-told story.

In spite of the presumed numerical strength of the Townsend and Coughlin movements and the support given Lemke by the leaders of those movements, the Union party candidate attracted less than one million votes. Bennett suggests a number of factors to explain why the party did so poorly: failure of organization, inability to get on the ballot in a number of states, lack of cooperation among the leaders, Lemke's weakness as a campaigner, improvement in economic conditions, and the charismatic appeal of Franklin Roosevelt. The emphasis, however, is not on the causes of defeat but on the party's significance, the sources of its support, and its relationship to earlier and later protest movements. The last of these points is the subject of a thoughtful and provocative epilogue: "Yesterday's Radicals and American History." The author analyzes various theses—neopopulism, isolationism, ethnic loyalty, and religious prejudice—which have been advanced to explain the appeal (such as it was) of the Union party. Without fully accepting any of these explanations, Bennett acknowledges that each has some validity since the leaders of the party were casting "a wide net among angry and frustrated Americans who might seek to solve their depression-bred woes in one stroke."

Well-chosen illustrations add to the value of this informative and highly readable study.

Ohio State University

ROBERT H. BREMNER

ESSAYS ON THE NEW DEAL. By *Wilmon H. Droze et al.* Foreword by *C. B. Smith*. Edited by *Harold M. Hollingsworth* and *William F. Holmes*. [The Walter Prescott Webb Memorial Lectures, Number 2.] (Austin: University of Texas Press for the University of Texas at Arlington. 1969. Pp. 115. \$3.95.)

THIS volume includes three memorial lectures dedicated to Walter Prescott Webb and given at the University of Texas, Arlington, in the spring of 1967.

Wilmon H. Droze ably chronicles the shelter belt project, which spent some fourteen million dollars to plant 220,000,000 trees on the Plains between 1934 and 1942 at a cost of approximately seven cents for each tree. The project, Droze notes, was in Roosevelt's mind as early as September 1932; it was not merely a gimmick to capture the farm vote in 1934. The program also provided an excellent example of Roosevelt's political skill. He tried to maximize congressional support in setting it up, but when Congress refused to appropriate money for it in the later 1930's he casually used WPA

funds instead. Droze shows that the tree planters made some mistakes, but he concludes with a favorable assessment. His essay uses some primary sources and is clearly told.

George Wolfskill, in "New Deal Critics: Did They Miss the Point?" lauds the New Deal as a highly innovative attempt to "democratize industrial and finance capitalism." But Wolfskill also stresses its relativism and occasional inconsistency. Moreover, Roosevelt "was never able to make clear . . . where the New Deal was going, what it really meant to do." Critics, therefore, soon mounted a "massive opposition" which became the "principal obstacle that prevented Roosevelt from achieving the goals of the New Deal." The author is sometimes more argumentative than provocative, more rambling than precise, and it is not clear what "point" the critics missed or how they should have acted differently.

Scholars looking for the authoritative account of the "court-packing" plan of 1937 will turn to William E. Leuchtenburg's long essay. It is beautifully written, carefully reasoned, and filled with illuminating detail gleaned from far-flung and little-used archives. Leuchtenburg analyzes the forces opposing the plan, suggesting provocatively that Catholicism was a key variable in explaining opposition. He concludes that the plan blunted the drive for social reform, divided state Democratic parties, cost Roosevelt invaluable middle-class support, undermined bipartisan backing for the New Deal, and helped create distrust for the administration's foreign policy. Yet, Leuchtenburg decides, the court proposal had one big success in legitimizing the vast expansion of American government. While these conclusions are not startling, they are so carefully documented and gracefully argued that they should remain definitive.

Indiana University

JAMES T. PATTERSON

ALL BUT THE PEOPLE: FRANKLIN D. ROOSEVELT AND HIS CRITICS, 1933-39. By *George Wolfskill* and *John A. Hudson*. (New York: Macmillan Company. 1969. Pp. xii, 386. \$7.95.)

FRANKLIN Roosevelt felt a special kind of identification with Andrew Jackson. It is fitting, then, that the title of this book about FDR's critics should be taken from one of his speeches about Jackson: "It seemed that sometimes all were against him—all but the people of the United States."

George Wolfskill, historian, and John A. Hudson, librarian, have collaborated to produce this study. They tell us that the collaboration strengthened their friendship, but they do not reveal how they divided their labors. No matter; the result is impressive. *All but the People* is a comprehensive examination of the Roosevelt critics, what they criticized and why, and "what (if anything) they tried to do about it." The authors have imposed two substantial limits on their efforts: they treat only of domestic affairs, and they restrict themselves to the period 1933-1939. They mention a third limitation, which forced a smile from me: deliberate omission of the "indecent and obscene." As the included attacks on FDR reek with vile charges of traitor, wastrel, monster, lunatic, fascist, subversive, dictator, Red, criminal, liar, Judas, tyrant, one wonders what the authors consider indecent and obscene!

The organization of the book is topical and logical. It ranges from the lowest and most personal kind of attacks (whisperings) to blisterings from Right and Left extremists and to outcries from the business community, the press, and the major political parties ("respectable" criticism). The authors display extraordinary industry and

skill in assembling their materials from a variety of sources and in fitting them into a lively, hard-hitting narrative. At times the materials, perhaps because of their sheer idiocy, grow a bit tiresome. The authors' point could often have been made with fewer examples. Stricter editing might well have reduced the length of the book by a third while sharpening its impact.

All but the People adds both to our knowledge of the New Deal and to our understanding of the nature of democratic politics. One cannot fail to be depressed by reading once again the vicious and erroneous criticisms of FDR and his administration. The attacks, for the most part, seemed stupid and twisted to many informed observers when they were first made; with a historian's perspective (some thirty years later) they appear still worse. Wolfskill and Hudson manfully attempt to explain why the critics felt and said what they did. But the criticisms, from uneducated and educated alike, remain a disgraceful stain on the democratic record. Further, one has the nagging feeling that major segments of the electorate today bear the same fetters of ignorance, irrationality, gullibility, and narrow self-interest.

The authors assert, in their conclusion, that FDR fell short of furnishing remedies for the serious ills of American society. Because of the "massive opposition," the New Deal never became much more than "a spirited evasion of the overriding issues of the twentieth century." And some of the opposition, they claim, arose from Roosevelt's failure to formulate and express a clear philosophy—to answer the crucial question: "What was the New Deal?"

I challenge both assertions. Roosevelt did provide remedies and new directions for democratic capitalism in the United States. All systems and remedies are, of course, dynamic; they cannot be for all time. Capitalism and democracy still contain some profound contradictions and defects; Roosevelt did not try to remove them and could not have removed them. What he did was to bring about crucial *adjustments* that made the system more viable and humane. No mean accomplishment.

And he had a philosophy for the New Deal—not in the formal or academic sense, but, nonetheless, in a meaningful sense. His critics charged that his course was wrong-headed—or nonexistent—but the majority of the voters got the message straight. They knew, in broad terms, what the New Deal was about. These are important points for the accuracy of the historical record, points amply documented in my book, *What Roosevelt Thought* (1958). Wolfskill and Hudson do not cite and may not have consulted this book, which focuses upon the precise questions raised in their conclusion.

Though the final section of *All but the People* is in part disappointing, the book is otherwise first-class historiography and should prove "definitive" for the subject. I, for one, have now "had enough" from and about the Roosevelt detractors!

Michigan State University

THOMAS H. GREER

MR. JUSTICE JACKSON: FOUR LECTURES IN HIS HONOR. By *Charles S. Desmond et al.* [Delivered under the auspices of the Association of the Bar of the City of New York and the William Nelson Cromwell Foundation. Legal Studies of the William Nelson Cromwell Foundation.] (New York: Columbia University Press. 1969. Pp. 136. \$5.00.)

ROBERT H. Jackson was the New Deal's supreme advocate, a Supreme Court justice of exceptional force and sparkle, and a driving spirit in the Nuremberg war crimes trials. No biography to date fully evaluates his career. Eugene Gerhart's *America's Advocate* (1958), though offering rich documentary material, is basically an apologia.

Glendon Schubert's *Dispassionate Justice* (1969), though offering solid evaluation, is basically an anthology of opinions. The present volume does not pretend to fill the void. It memorializes Jackson through four lectures by former associates whose aim is to refresh the memories of Jackson's contemporaries and to remind a new generation of his personal, professional, and judicial qualities. The lectures include: "The Role of the Country Lawyer in the Organized Bar and the Development of Law," by Judge Charles S. Desmond; "Mr. Justice Jackson and Individual Rights," by Paul A. Freund; "Robert H. Jackson's Influence on Federal-State Relationships," by Justice Potter Stewart; and "Robert H. Jackson's Contributions during the Nuremberg Trial," by Lord Shawcross.

Given their scope and purpose, the lectures are uniformly successful. While certain omissions are inevitable (for example, Jackson's intense and interesting views on the establishment and full faith and credit clauses), on balance the themes are well chosen and well executed. Standing out are Desmond's help in explaining why Jackson became the "lawyer's judge" of his generation, the succinct analyses by Freund and Stewart of complex constitutional positions, and Shawcross' assessment of Jackson's role at Nuremberg, which provides new clues about British attitudes toward the affair that Jackson regarded as his most constructive work. A bonus is the delightful personal portrait of Jackson by John Lord O'Brian.

Mercifully brief and often witty, the lectures are valuable to historians for two reasons: they provide insight into Jackson's personality and career as perceived by his associates, and they mirror the values of a professional elite in mid-century, values that Jackson himself often personified. To quote England's present Attorney General: "My last memory of Bob is of lunching in his room at the Supreme Court with Felix Frankfurter during the height of the McCarthy horror. There were the four flags from the Nuremberg Court room behind Bob's desk with the Hammer and Sickle of the Soviet flag unashamedly exposed." Thus do lawyers choose to remember him.

Johns Hopkins University

J. WOODFORD HOWARD, JR.

THE RECORDS OF A NATION: THEIR MANAGEMENT, PRESERVATION, AND USE. By H. G. Jones. With an introduction by Wayne C. Grover. (New York: Atheneum. 1969. Pp. xviii, 308. \$12.95.)

THIS study of the history, functions, and services of the National Archives and Records Service will be a revelation to most historians. Founded as recently as 1934, the National Archives advanced in relatively few years to eminence and in some fields to world leadership. Few even among its patrons will be aware of the full range and scale of the manifold activities described in this volume.

Anxiety regarding the future of the Archives prompted the survey upon which this book is based. There is deep concern about its status, which, since it lost its independence in 1949, has been simply that of a branch of the General Services Administration, and about the possibility that the National Archives and the Records Service might be separated. A Joint Committee on the Status of the National Archives was set up in 1967 by the American Historical Association, the Organization of American Historians, and the Society of American Archivists; Julian P. Boyd was appointed chairman and H. G. Jones, secretary. The need for a comprehensive background study was at once apparent, and this Jones was asked to prepare.

Records management is the least understood aspect of the National Archives. Little is known about it outside the institution because in most respects it is an in-

ternal service performed for other branches of the government—a highly efficient and economical housekeeping aid that saves the taxpayer many millions of dollars every year. But it is much more than this. It is a service vital to the historian for it ensures that records essential for research find their way to the Archives. Early in his career Robert Connor, the first Archivist of the United States, realized that the Archives could not be simply a passive receiver of old documents, that it must penetrate all branches of the government in order to make sure that records of long-term importance were identified and their ultimate transfer to the Archives assured. The scale upon which new records were even then accumulating was frightening, and their bulk was posing formidable problems. New management concepts were needed, and in 1938 Connor was already talking about “efficient appraisal,” the “destruction of useless material,” and substantial “savings in dollars.”

Solon Buck, the second Archivist of the United States, developed these concepts and instituted services. In its simplest terms, records management has two aims: first, to identify, at the earliest possible moment, records of long-term value and to arrange for their segregation and preservation; second, to provide for the systematic disposal of ephemeral material as soon as it has served its immediate purpose. The Records Disposal Act of 1943 reflected these ideas and authorized the use of continuing disposal schedules to ensure that large quantities of superfluous records would be disposed of virtually automatically.

All this happened years before the Hoover Commission, in 1948, asked Emmett J. Leahy to prepare a report and make recommendations on records management. Leahy, a former employee of the National Archives and a management enthusiast with great drive and ability, had left government service to develop records management as a business pursuit. To him, management was all-important, and, instead of being under the control of the Archives, he recommended that the Archives should be under a federal records administrator. Things were not carried quite that far, but, in 1949, the Archives—renamed the National Archives and Records Service—lost its independent status and was made part of the new General Services Administration. In 1950 the Federal Records Act gave to the Administrator of General Services virtually all the authority over records that had been vested in the Archivist of the United States. The Archivist himself was to be appointed thenceforth by the Administrator and not by the President of the United States.

There was a surface logic to the move; records management sounded like something that could be placed logically in the charge of the agency responsible for the business affairs of the government. But this view overlooked entirely the vital archival aspect and the attendant appraisal services that only the Archives and its trained staff could provide. Loss of its independence was a bitter blow to the prestige of the Archives; even more serious was the possibility that the National Archives and the Records Service might at any moment be torn asunder. Successive administrators have had the good sense not to do this, but the threat persists, and the results could be little short of catastrophic. The present study makes this crystal clear, and the remedy recommended is the restoration of the independent status initially and properly given to the National Archives in 1934—a change that would automatically protect it against dismemberment.

Ottawa, Canada

W. KAYE LAMB

BETWEEN THE BULLET AND THE LIE: AMERICAN VOLUNTEERS IN THE SPANISH CIVIL WAR. By Cecil Eby. (New York: Holt, Rinehart and Winston. 1969. Pp. xxi, 342. \$7.95.)

CECIL Eby, professor of English at the University of Michigan, has become one of the world's leading Spanish Civil War buffs. During the course of two Fulbright lectureships in American literature in Spain, he has researched a pair of extremely well-written narratives. The first, *The Siege of the Alcázar* (1965), provided the best reconstruction to date of a minor epic of that struggle. It was focused mainly on the Nationalist defenders, whereas this new work, whose title is taken from a verse by George Orwell, deals with the saga of American volunteers for the International Brigades on the opposing side.

This volume has the same merits as Eby's first narrative: it is well researched, nonpartisan in tone, and comparatively impartial in judgment. The account is based on the extensive published material dealing with the general area of the topic, considerable investigation in a variety of documentary sources, and numerous interviews with surviving veterans. As in the earlier book, Eby does not for the most part write in the manner of a professional historian, but in the anecdotal reportorial style of the "creative narrative"; skillful personality sketches have been penned in for nearly all the main figures in the battalion. As in *The Siege of the Alcázar*, numerous petty details might prove hard to substantiate, and the prose occasionally becomes a bit purple. The book has the advantage of being vivid and fast moving, and is well controlled in terms of basic perspective and major facts (as distinct from minor details).

Eby's book expands our knowledge of the men and deeds of the Abraham Lincoln Battalion (frequently mislabeled "Brigade"), and is in every way superior to two previous books on the subject, both of them partisan and propagandistic. Though this is mainly a descriptive account of the battalion in Spain, it is not deficient in political analysis. The author deals perceptively with the domestic American political background and the aftermath of the volunteers, including their association with the Comintern. He strikes an accurate balance between the political idealism of many if not all of the volunteers and the cynical manipulation of their effort, and for that matter of the whole Spanish struggle, by the Communists. His book will now become the standard narrative account of the Lincoln Battalion in Spain.

University of Wisconsin, Madison

STANLEY G. PAYNE

THE PRESIDENT AND PUBLIC OPINION: LEADERSHIP IN FOREIGN AFFAIRS. By Manfred Landecker. (Washington, D. C.: Public Affairs Press. 1968. Pp. v, 133. \$4.50.)

THE purpose of this book is to determine how Presidents Roosevelt and Truman "assessed and aroused public opinion in the formulation of foreign policy during two climactic periods in recent American history." The author concedes that the effect of public opinion on presidential decision is "difficult to analyze and frustrating to evaluate because of its inherent intangibility." Nonetheless he valiantly follows Roosevelt through the debate of the late 1930's over the American responsibility, if any, in the resistance to Nazism and then turns with some relief to the more straightforward problems of Truman in the 1940's in mobilizing opinion in support of the containment of Communism. His main sources are published letters and

memoirs, though he has found useful manuscript material in the Roosevelt and Truman Libraries.

As a political scientist, Professor Landecker is committed to the effort to transform history into a set of theoretical propositions. Unfortunately some problems do not benefit particularly from extended theoretical formulation, and public opinion seems to fall in this category. To quote Christian A. Herter as saying that "a successful foreign policy must . . . , to be effective, command the support of the vast majority of the American people" or to write that "the task of gaining public support is eased when the public is emotionally involved and is convinced that the policy and goals are commensurate with the potential risks" does not forward the analytical case very much. Most of the theoretical trimmings of this book consist of self-evident statements that the historian, whose professional deformation may well be undue impatience with superfluous abstractions, would not think of bothering to make. The sad fact is that the disquisitions on public opinion by political scientists and sociologists over the last half century have not notably improved on Walter Lippmann's book of 1922.

On the theoretical level this work would have gained if the author had carried further his intermittent effort to distinguish among the several publics that might conceivably influence presidential policy. Thus there is little attention here to business, labor, farm, ethnic, or academic organizations and only fitful recognition of V. O. Key's conception of the "opinion élite." Little use has been made of recent techniques of the analysis of presidential mail. Even within the book's theoretical scheme, the concentration on the executive-public relationship represents a fallacious abstraction. The vital relationship is triangular—the President, public opinion, and events—and events educate public opinion far more than Presidents can. What an effective President does is to offer the public an interpretation of their own experience, and a perennial frustration of the presidency is that interpretation offered in advance of events is not likely to be persuasive. To look at more recent history, it would be wrong to say, for example, that the Senate Foreign Relations Committee changed the minds of the American people about the Vietnamese War. The Vietcong and Ho Chi Minh changed the minds of the American people. What Senator Fulbright and his colleagues did was to provide an interpretation of the way things were going in Vietnam which, as time went by, made more sense than the alternative interpretation provided by President Johnson and Secretary Rusk, or their successors.

The factual material in the book is somewhat random but generally interesting and accurate (though Landecker repeats the myth, effectively disposed of by Dorothy Borg, that "reaction against the quarantine idea was quick and violent," and he also confuses William O. Douglas with Lewis W. Douglas and makes other minor errors). In contrasting Roosevelt and Truman, Landecker correctly notes that the great difference in the public mood on international issues between the thirties and the forties made Truman's task much easier. But *The President and Public Opinion*, no doubt because of the inherent difficulty of the problem, fails to advance the theoretical understanding of the influence of public opinion on presidential decision, and it is too brief and superficial to offer anything novel on the specific experiences of Presidents Roosevelt and Truman.

City University of New York

ARTHUR SCHLESINGER, JR.

DESEGREGATION OF THE U.S. ARMED FORCES: FIGHTING ON TWO FRONTS, 1939-1953. By *Richard M. Dalfiume*. (Columbia: University of Missouri Press. 1969. Pp. viii, 252. \$6.00.)

Moving beyond Ulysses Lee's description of the army's World War II racial policies and Lee Nichols' breezy account of integration of the armed forces, Richard Dalfiume has provided a useful analysis of changes in military racial policies and of the demands among Negroes for integration and equal opportunity in the armed forces. He has gathered considerable evidence that the war years (as he stated in the *Journal of American History*, LV [June 1968], 90-106) are "the 'forgotten years' of the Negro revolution," the watershed of recent Negro history. In the period before Pearl Harbor, as well as during the war, he found evidence of outbursts and protests against discrimination, and of demands and expectations for integration, that most historians have overlooked. In effect, Negroes then and afterward were fighting for democracy on two fronts—at home and abroad.

Though his treatment of Roosevelt is brief but critical, his analysis of Truman is more complete and quite favorable. He notes Truman's commitment to legal equality (but not social equality) for Negroes during his senatorial years and subscribes to the familiar interpretation that the new President's beliefs changed after he entered the White House. In explaining this shift, Dalfiume emphasizes Truman's "democratic idealism" and acknowledges the pressures of domestic politics and international events. On the basis of dubious evidence, the author claims that, by January 1948, the administration had decided to end segregation in the armed forces. But the effort to retain southern support delayed for seven months the executive order ultimately leading to desegregation of the military. Unfortunately, he never explains why that order specifically barred only discrimination and not segregation. And he goes badly astray by using an ambiguous newspaper report, rather than the available transcript, of a presidential press conference to conclude (wrongly) that Truman had declared earlier that segregation was a form of discrimination.

In other places, too, the research is incomplete, and the analysis is limited. For example, Dalfiume never considers why a President who was presumably committed to civil rights could allow the army in Europe to delay starting desegregation for nearly two years after the original order. Perhaps the theory of Truman's commitment warrants reconsideration. An analysis of other aspects of the civil rights program might indicate that the administration frequently asked Congress for more than it would grant, but temporized in those areas where executive power was sufficient to revise racial policies. Also, the author's neglect of colonialism as an important postwar issue means that he never adequately considers why, by the election of 1948, Negroes seemed unquestionably to accept the cold war and military service on behalf of "freedom." Surprisingly, in addition, he seldom distinguishes between Negro leaders and the masses, and he sometimes seems to forget that whites dominated the leadership of many civil rights organizations. Furthermore, despite considerable research at the Truman Library and in the relevant military collections at the National Archives, he fails to use the records of the NAACP, the Schomburg Collection, the oral history memoir of Roy Wilkins, and the papers of Clark Clifford, Walter White, Francis Matthews, Frank Pace, and Robert Patterson. Even with these shortcomings, however, this monograph is a creditable contribution to an understanding of what may be the most neglected period of domestic history.

Stanford University

BARTON J. BERNSTEIN

THE POLITICS OF WAR: THE WORLD AND UNITED STATES FOREIGN POLICY, 1943-1945. By *Gabriel Kolko*. (New York: Random House. 1968. Pp. x, 685. \$12.95.)

THE ROOTS OF AMERICAN FOREIGN POLICY: AN ANALYSIS OF POWER AND PURPOSE. By *Gabriel Kolko*. (Boston: Beacon Press. 1969. Pp. xviii, 166. \$5.95.)

In analyzing the political events of World War II, Professor Kolko places the emphasis on economic and social factors rather than on those of diplomacy and power politics. The result is an original work that stresses a dimension often ignored in examining the interaction of American, British, and Soviet policies during those crucial years.

The Politics of War pictures, for instance, the role of the anti-Nazi resistance in Italy and France as one that normally would have entitled the Left to take over the reins of government; the role of the Western Allies in excluding the Soviets from influence on internal Italian developments is seen as essentially resulting from economic and social preconceptions. In fact, Kolko sees the Russian role as an essentially moderating, "antirevolutionary" one. (It is true, of course, that after liberation the Western Communist parties subordinated everything to support of the war effort.)

The same, *a fortiori*, is his conclusion with respect to the Eastern European states that came under Soviet occupation. Far from seeing the Soviets as trying to force Communist regimes on those countries for reasons of national security or ideology, the author pictures them as trying essentially to let normal events take their course. He sees the role of the Western Allies as opposing those normal tendencies because of an anti-Left prejudice derived principally from economic self-interest.

The author covers the entire gamut of wartime diplomacy, both in Europe and the Far East, from 1943 to the Potsdam Conference. The book includes discussion of the economic relations between the Allies, neglecting neither the wartime confrontation in Iran, nor the Soviet application for a loan for postwar reconstruction, nor, of course, the reparations problem and its repercussions on the occupation arrangements for Germany.

There are some highly original conclusions: for instance, that Stalin did not really pursue a policy involving spheres of influence (Churchill's deal with him in October 1944 is seen as a case of Stalin merely humoring the British leader); that the Morgenthau plan was even more directed against the USSR, by depriving it of reparations, than against Germany; and that events proved that the Soviets really had no control over the Western European Communist parties.

Despite the fact that the author makes some attempts at objectivity, his basic thesis obliges him to de-emphasize certain events and to highlight others. A simple litmus test is the Katyn affair, where he says at first: "The criminological evidence aside—for it is vast and has been used convincingly to prove the culpability of both sides—certain larger facts must be taken into account." (Some remarks follow about the undoubted unwisdom of the London Poles in asking the Red Cross to investigate the Nazi charges of Soviet atrocities.)

On the next page, we find a different attempt to minimize the crime: "If the criminological evidence exhumed in this ghastly affair does suggest Russian guilt, or if the Russian defense is inconsistent on serious points, then it must be suggested that Katyn was the exception rather than the rule. . . ." Some readers may find such strained language embarrassing. It is true, however, that the Katyn massacre was not the principal determinant of the breakdown in Russian-Polish relations during the war; other factors were more important.

Implicit in the author's presentation of facts and opinions is the assumption that the Communists ("the Left") represented authentic nationalist elements both in Western and Eastern Europe during the wartime period. This was undoubtedly true to a large degree in Western Europe, but the author does not consider the possibility that the Communists may have represented only a minority. Or, if he does consider it, he does not consider it important. He denies that conservatives could have been authentic nationalists and ignores the emergence of liberal, anti-Communist nationalists. The confrontation is drawn in black and white: misdeeds of the Americans and British are detailed; misdeeds of the other side are often ignored or minimized.

The Roots of American Foreign Policy shows Kolko in the different role of an angry pamphleteer. Here his economic interpretation of American foreign policy is carried to the extreme of describing Vietnam as a war for the survival of American capitalism. For this reason, the author fully subscribes to the domino theory: "What is at stake, according to the 'domino' theory with which Washington accurately perceives the world, is the control of Viet-Nam's neighbors, Southeast Asia and, ultimately, Latin America."

Whereas *The Politics of War* represents a respectable intellectual effort, though one flawed by overanxiousness to find simple explanations for a multiplicity of actions and reactions, the smaller, later volume is essentially a piece of journalism. It must be painful to historians to see themselves neatly divided into "conventional" and "Left scholars." Kolko leaves no doubt into which rubric he places himself.

One may deplore or oppose the war in Vietnam for varied reasons, but it is difficult to dismiss, as "tendentious reasoning," the Kennedy administration's documentary proofs that the National Liberation Front had been formed by the Lao Dong party and that the guerrilla war in 1960 had been launched by that party, that is, by South Vietnam's neighbor to the north. To go so far as to speak of a "myth" of North Vietnamese intervention amounts to ignoring one important dimension of that tragic conflict.

United States Foreign Service

MARTIN F. HERZ

THE TRUMAN ADMINISTRATION AND THE PROBLEMS OF POSTWAR LABOR, 1945-1948. By *Arthur F. McClure*. (Rutherford, N.J.: Fairleigh Dickinson University Press. 1969. Pp. 267. \$8.00.)

THIS book deals with the shaping of a major phase of contemporary American labor history. The emergence of big unionism under the aegis of federal legislation during the 1930's inevitably raised labor-management relations to a political issue of the first order. The formation of public policy would have been painful at any time, for both labor and management held explicitly voluntaristic ideologies (both worn somewhat thinner, to be sure, by the New Deal experience), and, excepting for its experience with railroad labor relations, the federal government had to start nearly from scratch. Taking place as it did in the turbulent postwar years, the policy-making process became immensely more complicated than it would otherwise have been.

Professor McClure's book does not measure up to this demanding subject. The work does lay out the main line of development, and it usefully details the legislative groundwork for the Taft-Hartley Act. But little attention is devoted to the crucial crosscurrents—the interplay of a labor movement divided into warring camps; a business establishment still uncertain about whether and how to engage in collective bargaining; a controlled economy now being unfettered and verging on an inflation-

ary orgy; a public opinion furious with organized labor for ostensibly obstructing the war effort and, still worse, the abundance of peacetime; and political parties seeking at once to win elections and formulate responsible policy. The little attention they do receive is sadly lacking in sophistication. The fault probably arises in part from thin research. McClure did not venture very far into the voluminous trade and labor periodical literature, or into the available manuscript collections. Granting the limitations at the Truman Library, there remain the materials at the National Archives that he barely skimmed and the AFL and CIO records that he did not touch at all. This book will serve a useful function until a more authoritative work comes along, but readers would do well to go to Barton Bernstein's fine articles for the topics covered by them, and also to continue to utilize Joel Seidman's early *American Labor from Defense to Reconversion* (1953).

University of California, Davis

DAVID BRODY

THE KOREAN WAR AND AMERICAN POLITICS: THE REPUBLICAN PARTY AS A CASE STUDY. By *Ronald J. Caridi*. (Philadelphia: University of Pennsylvania Press. 1968. Pp. 319. \$10.00.)

THIS is a history of the policy of the Republican party toward the Korean War. The nine chapters span the period from the time before June 1950 to the end of the conflict during the Eisenhower administration in 1953, including the various phases of the conflict such as the early success of the North Koreans, the United Nations offensive in the fall of 1950, the crossing of the thirty-eighth parallel, the entry of the Chinese Communists into the war, the removal of General Douglas MacArthur, and the final settlement.

The author is aware of the difficulty of discussing the policy of the Republican party or any party, particularly when it controls neither the White House nor Congress. In using the terms "Republican attitude" or "Republican reaction" he carefully notes the differences of leading Republicans on virtually all issues. Yet he believes that the majority of Republican leaders in and out of Congress did constitute a group that could be considered the Republican party for the purposes of this study.

His yardstick for judgment is that suggested by Norman A. Graebner: "The real test of wisdom that confronts any party which attempts to expand its influence through the successful use of foreign policy symbols is whether its assumptions will form the basis of responsible alternatives that recognize national limitations." Professor Caridi believes that the Republican party failed the test as the "party was neither consistent nor sincere in its development of an alternative to the policies formulated by Truman and the Democratic Party." His conclusion, based on the assumption of the proper role of a political party out of power, is valid. Yet one wonders whether most politicians in any party, with an eye to the next election, have ever acted very differently from the Republicans during those years.

The volume is written almost entirely from published materials, chiefly the *Congressional Record* and the *New York Times*. The book is well organized and clearly written, and the author has achieved the purpose expressed in the title. His research of published materials is thorough, but the reader is sometimes distracted by the rather numerous and lengthy quotations. This book certainly should be consulted by anyone interested in the influence of the Korean War on American politics.

California State College, Los Angeles

EDWARD O. GUERRANT

THE FRANCHISE AND POLITICS IN BRITISH NORTH AMERICA, 1755-1867. By *John Garner*. [Canadian Studies in History and Government, Number 13.] ([Toronto:] University of Toronto Press. 1969. Pp. 258. \$7.50.)

PROFESSOR Garner's study of the franchise in the British provinces prior to Confederation is the first connected account of an important aspect of the politics of a period that many would regard as having been thoroughly investigated by several generations of historians. By adopting the British system whereby the right to vote was conferred on all landowners, a very wide system of suffrage existed in the rural areas of British North America where the majority of the population held land under the generous system of land grants designed to attract immigrants. In later years, however, the rise of urban centers, the appearance of leaseholders (particularly in Prince Edward Island), the arrival of foreigners who were not immediately naturalized, and the appearance of farm laborers created a situation that soon produced bitter political controversy in all the provinces. In treating this subject in meticulous detail, Garner has undoubtedly produced a definitive account that will survive the passing years as a source for Canadian historians and political scientists.

Following a general discussion of the franchise in Britain, Canada, and the United States, the study treats each province in turn. This is followed by chapters dealing with religious disqualifications in the franchise, with the position of minors, women, and Indians, then with aliens and naturalization, and, finally, with two chapters on controverted elections.

A considerable amount of information on the franchise and on the establishment of representative assemblies in the colonies has already been thoroughly investigated in existing accounts, but the author has re-examined the sources to present the evidence in the context of the franchise question.

So far as public opinion is concerned, British North Americans displayed that conservatism that has been a distinguishing feature of Canadian political life. Except for radical minorities, no party in the provinces espoused manhood suffrage, which was identified with American democracy. The colonists' deep attachment to British constitutional practice provides another example of the inapplicability of the frontier thesis to Canadian political and social development.

University of Alberta

LEWIS H. THOMAS

LES LA VÉRENDRYE ET LE POSTE DE L'OUEST. By *Antoine Champagne*. [Les cahiers de l'Institut d'Histoire, Number 12.] (Quebec: Presses de l'Université Laval. 1968. Pp. x, 589. \$12.00.)

THE author of this book provides students of history with a detailed examination of the role played by Pierre de La Vérendrye, his four sons, and his nephew La Jemmeraye in the exploration of the Northwest. After an introductory chapter in which he describes the state of La Vérendrye studies, Antoine Champagne neatly divides his presentation into three principal parts: "Avant la conquête de l'Ouest"; "Les La Vérendrye et la conquête de l'Ouest, 1731-1749"; and "Les Successeurs de La Vérendrye et les dernières années de l'Ouest français, 1750-1761." His investigation is complemented by three appendixes dealing with the financial difficulties of La Vérendrye, his last operations, and the subsequent penetration of the French into the heart of North America. It also includes an impressive bibliography, a most useful index of proper names, and an analytical table of contents. The term "Poste de l'Ouest," not explicitly defined until pages 261-62, was first applied to Fort Kaminis-

tiquia, built in 1717 where the city of Fort William, Ontario, now stands, but it was also used to designate seven other forts that the La Vérendryes erected later, in the course of their search for the Western Sea.

Although the author retraces the activities of the six members of the La Vérendrye family who participated in that momentous undertaking, he particularly stresses the career of the most illustrious of them, that is, Pierre de La Vérendrye. Unsuccessful in his efforts to make his dream—the discovery of the Western Sea—come true, he did, however, in the pursuit of that dream, annex to France's possessions vast territories stretching from Lake Superior to the Saskatchewan River and the Dakotas. By the judicious use of extensive documentation drawn from both primary and secondary sources, Champagne has added in notable measure to our knowledge concerning the La Vérendryes and the exploration of the Northwest. His book represents, therefore, a valuable contribution to North American historiography.

University of Virginia

JOSEPH MÉDARD CARRIÈRE

CANADA'S RMC: A HISTORY OF THE ROYAL MILITARY COLLEGE.

By *Richard Arthur Preston*. ([Toronto:] University of Toronto Press for the Royal Military College Club of Canada. 1969. Pp. xv, 415. \$10.00.)

THE Royal Military College at Kingston, Ontario, was established late in the nineteenth century to provide a small number of trained army officers to meet Canada's limited military needs. It was unique among military academies in that it was the first to be created in a colonial dependency and in that it had the dual mission of preparing its students for nongovernmental civilian careers as well as for military commissions. Aably launched under the forceful and imaginative leadership of its first commandant, Colonel Edward O. Hewett, it provided well-educated officers for Canada's permanent and nonpermanent peacetime militia, civilian engineers for the growing country who were available for military service in time of war, and a small group of graduates who pursued successful careers in the regular British Army. Closed during World War II, it reopened as a triservice academy in 1948, a role that it fills today in the new scheme of Canadian military affairs.

Although the RMC has been in existence for nearly one hundred years, this volume is the first published history of the school. The author, a professor of history at RMC from 1948 to 1965 and a former president of the Canadian Historical Association, has done a thorough job of describing the establishment, growth, and transformation of the college. He includes perceptive discussions of such varied matters as political pressures, financial difficulties, curriculum development, hazing, and faculty recruiting. And since he witnessed at close hand the post-World War II reorganization of the RMC, his description of this most recent period is doubly valuable.

While Professor Preston is proud of the Royal Military College and its accomplishments, he is by no means uncritical. He makes his own views quite evident, and this quality, combined with an eye for interesting anecdote, lends a nice, personalized touch to his account. Well-researched, carefully balanced, and with sufficient detail for the most curious reader, this book will remain the definitive study of the RMC for many years to come.

Industrial College of the Armed Forces

STANLEY L. FALK

ESTADOS UNIDOS Y AMÉRICA LATINA, SIGLO XIX. By *Manuel Medina Castro*. (Havana: Ediciones Casa de las Américas. 1968. Pp. 774.)

THIS prize-winning study (*Premio Ensayo* 1968, *Casa de las Américas*) is the work of *their* man in Havana, Manuel Medina Castro, an Ecuadorian now living in exile in Cuba. Its thesis is straightforward and simple: United States-Latin American policy during the nineteenth century was an unbroken chronicle of chicanery, greed, hypocrisy, and aggression. The policy described went beyond the mere pursuit of national self-interest; in the end it became gratuitously offensive. *Yanqui* malevolence, apparently, was and is a bottomless chasm.

Medina seems impressed by two things: the extent of his research and the need to rescue the history of US-Latin American relations from previous whitewash jobs. On the first point, documentation is sketchy, and the chief primary source is the published diplomatic correspondence. (The book is lamentably and unnecessarily long in relation to the data examined.) On the second point, Medina's knowledge and use of monographic material are almost nonexistent. The whole body of work by Americans critical of US foreign policy is ignored. Shortened and shorn of loaded modifiers, the book would fall into a well-known and intellectually respectable mold, best exemplified at the beginning of this century by Argentine attacks on the Monroe Doctrine, or the more evocative work of José Enrique Rodó, *Ariel*. But Medina is a "True Believer," and he considers himself a historiographical pioneer.

Perhaps it is no typographical error on page 283 where Medina calls the occupier of Monterey, "Tomás Ape Jones."

University of California, Los Angeles

FRANK OTTO GATELL

ZAPATA AND THE MEXICAN REVOLUTION. By *John Womack, Jr.* (New York: Alfred A. Knopf. 1969. Pp. xi, 435, xxi. \$10.00.)

ZAPATA: THE IDEOLOGY OF A PEASANT REVOLUTIONARY. By *Robert P. Millon*. (New York: International Publishers. 1969. Pp. 159. Cloth \$5.95, paper \$2.25.)

JOHN Womack's *Zapata* is an exciting, even haunting, book that anyone who presumes to know anything about the Mexican Revolution must read.

This book is not, and does not pretend to be, a biography of Zapata; the biographical data are incidental to the theme. This is a story, to use Womack's words, of a country people struggling to regain and to maintain a traditional way of life, with traditional values and virtues. As Womack tells it, using all the techniques of careful scholarship and excellent documentation, it is a story of pathos; it has few heroes, only a few villains, and many bewildered people caught in a net of fear, suspicion, and misunderstanding. Greed there was, and ambition and brutality, but when one finishes the book he wonders whether their presence had much effect, whether they were casual or incidental. Womack's style and his use of documentation give the era an aura of impending and rushing doom, of elemental forces in violent conflict which no one man, or small group of men, could halt until the whole dreary tragedy had played itself out.

Some readers might object to certain of Womack's interpretations. The Zapata adulators will find Zapata a man of integrity but of doubts, and no pristine hero leading his people to untarnished victory. Others will, perhaps, find other questionable points of view, but these are niggling and should be discussed quietly over tea, not made the subject of violent discourse through a review. The plain fact is that

Womack has shown himself to be an excellent scholar and a gifted writer, in spite of too frequent use of "deals," and he should have a brilliant career ahead of him.

Moving from Womack to Millon is not exactly an exciting experience. Mr. Millon, using only published documents and some secondary sources of doubtful value, concludes that Zapata's ideology was "radical petty-bourgeois" and "anti-imperialist," and therefore quite different from the Carranza ideology of conservative opportunism. He comes to this not too startling conclusion through an examination of Zapata's public pronouncements and manifestoes and insists that the thought in all such documents was Zapata's. In developing his thesis he is repetitive and sometimes contradictory, and he often gives strange interpretations: Zapata's insistence that foreign-held lands be subject to the same laws as national-held lands he says is evidence of Zapata's "anti-imperialism," for example. Furthermore, he accepts as truth every accusation made by Zapata against either Madero or Carranza. By using the same technique in reverse, one could just as easily conclude that Zapata was a reactionary and Carranza a pure, shining social reformer.

Womack's book is big and meaty; Millon's is small and lightweight.

Michigan State University

CHARLES C. CUMBERLAND

INTELLECTUAL PRECURSORS OF THE MEXICAN REVOLUTION, 1900-1913. By *James D. Cockcroft*. [Latin American Monographs, Number 14, Institute of Latin American Studies, University of Texas.] (Austin: University of Texas Press for the Institute. 1968. Pp. x, 329. \$8.50.)

MR. Cockcroft misleads us with his title. This is not a study of all the intellectual precursors, or even the most important; rather, it is an examination of the leadership of the *Partido Liberal Mexicano* and the shifting ideology of that group, most of whom came from San Luis Potosí. Madero is included as one of the precursors, but the exposition of his thinking is at best cursory. Occasionally the author interjects bits of information about dozens of others who qualify as precursors, but he makes no sustained examination of any of them. His thesis is that although Ricardo Flores Magón and other PLM leaders were eliminated as political figures, their ideas forged the Constitution of 1917.

In order to develop his thesis Cockcroft begins by discussing the social-economic structure in San Luis Potosí at the turn of the century and the national economic crisis in the first decade; his approach suggests a dependence upon a theory of economic determinism. He then describes in detail the formation of the various Liberal Clubs after 1900, and the ultimate emergence of the PLM and its Program of July 1906. He follows with a study of the various military movements under the aegis of the PLM and concludes by showing the split in the PLM leadership and the ideological position taken by many members between 1910 and 1917.

In the process of his work the author has accepted some questionable assumptions, the most important of which is that all PLM ideological statements were discrete and original and that, if the ideas finally became a part of the Constitution, they stemmed from a PLM source. In general, in his sympathy with his subject he gives too much importance to PLM military and ideological prowess.

Despite these weaknesses, this is an excellent book, carefully researched and well written. Cockcroft demonstrates, more clearly than any other author writing in English, the depth of dissatisfaction in the late days of the Díaz regime and the reasons for that discontent. He explains completely some of the shifts that took place,

and that too often have been mistaken for evidence of mere personal grasping for power. He shows with great clarity the real dilemma faced by many honest and honorable men and the ultimate consequences of their choice of a path. Unconsciously, perhaps, but completely, he proves that the ideological revolution that took place was the work of no one man or small group of men but an accretion of ideas coming from many men over many years.

Michigan State University

CHARLES C. CUMBERLAND

BLACK INTELLECTUALS COME TO POWER: THE RISE OF CREOLE NATIONALISM IN TRINIDAD & TOBAGO. By *Ivar Oxaal*. [International Studies in Political and Social Change, Number 3.] (Cambridge, Mass.: Schenkman Publishing Company. 1968. Pp. xiii, 194. \$7.95.)

THE emergence of a sense of nationalism in the British West Indies has yet to receive the attention it deserves either from historians, political scientists, or sociologists. Professor Gordon K. Lewis touched on the matter discursively but with insight in *The Growth of the Modern West Indies*, and this series, edited by Professor Wendell Bell, has initiated a more systematic investigation.

Dr. Oxaal is concerned primarily with the rise to power of Dr. Eric Williams and the People's National Movement, but he devotes the first half of his work to an account of the historical evolution of Trinidad and Tobago and to some of the distinctive characteristics of the country's social structure, without which it would be virtually impossible to understand the pattern of events in the last fifteen years. Although he treats the economic aspects of this history somewhat thinly, the social and political consequences of a dependent colony formed by the needs of its metropolitan nucleus are clearly presented. Oxaal argues cogently that Trinidadian politics and national feeling have been inhibited in their development by a notably heterogeneous ethnic structure: in the early 1950's, for instance, no less than thirteen endogamous ethnic structures were identified. However, two ethnic groups—Negro and East Indian—have been and remain politically decisive. Class divisions of course exist, and during the 1930's it seemed that they would determine the nature of the colony's political development, but, since World War II, ethnic identity has tended to dominate class feeling. It was out of this ethnically fragmented, pseudopluralistic society that Williams created that "remarkable marriage between the creole intellectual and the colonial crowd" that was to become the PNM. He now enjoys some fifteen years of virtually unchallenged power and can count among his achievements the winning of independence from Britain and a temporary accommodation with American power. Whether his essentially Negro-based party can meet the long-term needs of a society in which Negroes will soon cease to form the majority is, however, another matter.

Oxaal has handled this fascinating but complex and contradictory story with great sensitivity. It is a healthy sign that his obvious sympathy and admiration for the people of Trinidad and Tobago have not prevented him from some sharply critical comments when he felt they were warranted. One now looks forward to a comparative study of West Indian nationalist movements, which will relate social structure to political evolution as Oxaal has done so effectively here.

Sir George Williams University

ALAN H. ADAMSON

THE POLITICS OF INTERVENTION: THE MILITARY OCCUPATION OF CUBA, 1906-1909. By *Allan Reed Millett*. [Publication of the Mershon Center for Education in National Security.] ([Columbus:] Ohio State University Press. 1968. Pp. x, 306. \$6.50.)

CUBA: THE MAKING OF A REVOLUTION. By *Ramón Eduardo Ruiz*. ([Amherst:] University of Massachusetts Press. 1968. Pp. 190. \$6.00.)

IN this excellent study of the second US military intervention, Millett analyzes US efforts at political institution-building in Cuba. The career army officers in the Army of Pacification and the occupation government heavily influenced the course of US policy. Although they would have preferred a longer period of tutelage with broader reforms to create a "disciplined democracy," they did not favor annexation. They were also opposed to Magoon's attempt to prepare the Cubans for self-rule by laying the constitutional and practical foundations for a two-party system modeled after that of the US. The intervention was not a success; it further disintegrated the weak party system, produced a more efficient government apparatus "that was a far handier tool for waste and oppression," and created a permanent army that also served unscrupulous Cuban politicians after the United States withdrew.

This is an outstanding replacement for Lockmiller's work on Magoon, and it is superior in approach and use of Cuban sources to Healy's book on the first intervention. Despite some minor confusion about racial and class tensions in Cuba, the author has handled his themes adroitly, especially the complex military-civilian relations and the cross-purposes of US and Cuban officials. If history can teach, this volume should be richly instructive for those US officials wrestling with the aftermath of the recent Dominican intervention. The lesson of this Cuban case is compelling; interventions do not solve but rather increase local problems with unforeseen effects. Cubans have said that the second intervention laid the groundwork for later dysfunctions of the Cuban political system. Millett's book suggests they are correct.

Ruiz' study is of very different scope and purpose. His thesis is that Castroism can only be understood as a product of the past, for "the shape and course of the Revolution were determined primarily by internal circumstances which were products of a special society." Cuba differed from its Caribbean neighbors in its uneven mixture of relative advancement and serious problems. But Cubans measured their accomplishments against those of the United States because they identified with US values and they were slow to develop an integrative spirit of nationalism. Large foreign investments and the supremacy of sugar skewed economic development and led to radical demands for changes. The failure of the 1933 revolution increased unrest in this society afflicted with class and racial divisions, rural poverty, and political corruption. Ruiz demonstrates that the rise of Castro and his behavior in power are results of these conditions, rather than historical aberrations. Even Castro's overwhelming personal dominance is similar to that of earlier leaders, especially Martí, and his conversion to Communism is consistent with the strength of Leftist doctrines in Cuban life.

Ruiz has written an unequalled introduction to the Castro revolution. Many of the insights are not new, but they have never been brought together in such a coherent and complete way. This work will serve as an effective answer to the polemical and one-sided (though certainly brilliantly written) books of Draper. I disagree with Ruiz on a few points, however. Among them is Ruiz' acceptance of the false, but

widespread, belief that the peasants had little to do directly with the Castro guerrillas in 1957-1958.

Indiana University

DAVID D. BURKS

DICTATORSHIP AND DEVELOPMENT: THE METHODS OF CONTROL IN TRUJILLO'S DOMINICAN REPUBLIC. By *Howard J. Wiarda*. [Latin American Monographs, Second Series, Number 5.] (Gainesville: University of Florida Press. 1968. Pp. 224. \$3.75.)

PROFESSOR Wiarda's monograph represents a pioneering attempt to relate the thirty-one-year rule of General Rafael Leonidas Trujillo Molina to general theories of dictatorship and totalitarianism. To accomplish this, the author has undertaken a detailed analysis of the ways in which the Trujillo regime maintained its control over virtually all segments of Dominican society. The basic thesis of this study is that the Trujillo regime represented a transitional phase in the development of dictatorial governments, moving from a traditional, personalist military dictatorship in its early years to an increasingly totalitarian regime in the post-World War II period. This trend toward ever greater control over economic and social as well as political aspects of Dominican life reflected and, in some ways, encouraged the technological modernization of the nation.

Wiarda has uncovered little new material in support of his thesis. There is little evidence that he has made any use of the meager archival resources available. The chapters dealing with such subjects as Trujillo's control over the armed forces, the economy, the government bureaucracy, and the various means of public information including the educational system and the Church are based largely upon published works and personal interviews. Perhaps the most informative chapter is that dealing with Trujillo's political ideology and its place in the over-all structure of the regime. At times, as in the discussion of the events leading up to Trujillo's assumption of dictatorial powers, Wiarda's treatment seems somewhat cursory. In general, however, he has handled his material with considerable skill. If he furnishes us with few new facts about the regime, he has done an excellent job of drawing together the available data and organizing it so as to give us a better understanding of how Trujillo managed to transform the Dominican Republic from a highly volatile political society into the most totally controlled nation in Latin America. Within the limitations that he has set for himself, Wiarda has produced a significant study, of value to the general student of Latin American history as well as for those of us who concentrate upon the Caribbean area.

Southern Illinois University, Edwardsville

RICHARD L. MILLETT

* * * *

Communications

* * * *

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

In his notice of the late Desmond Ryan's *The Fenian Chief* (*AHR*, LXXV [Oct. 1969], 130), Professor Costigan makes the extraordinary statement that the book is based on printed sources. I can only conclude that he has not read the book he professes to review, since references to the manuscript material used will be found in the prefatory matter under "Acknowledgments" (p. vi), "Editorial Note" (p. viii), and "Bibliography and Sources" (p. ix), as well as in numerous footnotes throughout the book. The material includes the Stephens, Luby, Larcom, and O'Brien Papers (National Library of Ireland) and Stephens' American diary (Public Record Office, Northern Ireland).

Costigan's gross carelessness is to be doubly resented; it is insulting to Ryan's memory, and it is liable to give a totally false impression of the value of *The Fenian Chief*.

Mount Allison University

JOHN W. BOYLE

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

In the original handwritten draft of my review of *The Fenian Chief*, the final sentence read: "The book is based on manuscript and written sources and has a useful biographical index." In the typescript the words "manuscript and" were inadvertently omitted, and by an unaccountable oversight this typographical error went undetected in the proofreading of the galleys.

For this unfortunate lapse I must apologize to Professor Owen Dudley Edwards, the editor of the volume. I regret the error all the more since I have long admired the late Desmond Ryan as a man and as a scholar and could have no wish to minimize the value of his work.

University of Washington

GIOVANNI COSTIGAN

* * * *

Association Notes

* * * *

RECENT DEATHS

Charles Bradford Welles died suddenly at New Haven, Connecticut, October 8, 1969. He was born August 9, 1901, in Old Saybrook, the son of Charles T. and Edith Smith Welles. He married Eleanor Bogert; she and their two sons, Bradford W. and David W., survive him. He was a member of the Board of Editors of the *American Historical Review* from 1962 to 1967.

His career was very closely connected with Yale University, where he was professor of ancient history. He came to Yale from Phillips Exeter Academy in the fall of 1921 as a member of the Yale College class of 1924 and distinguished himself by winning ten classical prizes. Upon graduation he entered the Graduate School where he received his Ph.D. in 1928. He was appointed instructor in classics in 1927, assistant professor in 1939, professor in 1940.

As a graduate student he at once attracted the attention of Michael Rostovtzeff, whose coming to New Haven in 1926 had so remarkable an effect on the study of the classics at Yale. It was a propitious time, for Yale had undertaken to devote a large grant from the General Education Board to excavation, and in the choice of a site the personalities concerned were of the utmost importance. Franz Cumont, the eminent Belgian archaeologist, had been excavating an ancient city on the Euphrates, known by the Semitic name, Dura, or the Macedonian one, Europos. Though his resources were scanty, Cumont achieved remarkable results, and when it was necessary to bring his expedition to an end, it was obvious that he had added to the world's archaeological assets a town whose Greek, Roman, and Parthian remains were of unique importance. Cumont and Rostovtzeff were old friends, and it was through their collaboration that arrangements were made for Yale, in conjunction with the French *Académie des Inscriptions et Belles-Lettres*, to continue the unfinished work. The results were remarkable in many fields, but it was the abundance of inscriptions and perhaps still more the papyri that excited Rostovtzeff and gave Welles so much to do that Dura may be considered the focus of his scholarly career.

The original plan had been to issue annual preliminary reports of the year's works, leaving more elaborate discussion of the various categories to a series of final reports to be produced when the dig was finished. But Rostovtzeff's idea of a preliminary report was so very extensive, particularly in the discussions of buildings excavated, that it seems most unlikely that they will be replaced. In the work of producing these "Preliminary Reports," Welles showed his great versatility. It was not until the report on the "Sixth Season of Work (1932-33)" that his name appears on the title page as one of the editors, but long before that he had proved himself invaluable. He was an admirable complement to Rostovtzeff whose largeness of vision sometimes led to exuberance and haste. Welles was scrupulous, respectful of details, spending any amount of time to see that an item, no matter how apparently trivial, had the most accurate and thorough treatment possible. The "Final Report on Parchments and Papyri" on which he worked

in collaboration with Robert O. Fink and J. Frank Gilliam is a monument of learning and meticulous care, a glory to the memory of Cumont and Rostovtzeff to whom it is dedicated, and a memorial of the scholarship of its authors.

It must not be thought that Welles's attention was given to papyri and parchments alone. He never forgot his primary obligation of instruction, and the enthusiasm of his many graduate students testifies to his success. In addition, he had an independent military career as a field artillery officer, which took him as a major to Cairo, where he worked with the British from 1944 to 1946, and was decorated with the Order of the British Empire. Later, during the Korean War, he served at the Pentagon as a full colonel. Fortunately for him, on both tours of duty he was involved in affairs of the Middle East so that what might have been mere interruptions actually deepened and enriched his knowledge of territory vital to his professional career.

He was essentially and always a specialist and might well have considered as his major achievement the founding under his auspices of the American Society of Papyrologists in 1961 with the consequence, unsuspected by him, that nineteen of his associates, at their meeting in Toledo in 1967, presented him with a volume of *Essays in Honor of C. Bradford Welles*.

Yale University

ALFRED R. BELLINGER

The Reverend Charles E. Schroder of Detroit, Michigan, died November 1.

Other members of the Association who died recently are: Susie M. Ames of Pungoteague, Virginia, and John Brennan of Olean, New York.

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This volume, however, tries to do more than show physical development—it attempts to suggest how the city expanded and why it looks the way it does. This broad purpose explains the dual authorship, for the task required the tools of both the historian and the geographer—the former with his emphasis on the texture of life and the latter with his concern for spatial relationships. It is the joining of the land and the people that makes the city, and the partnership of two disciplines was felt to be an appropriate strategy in approaching this process.

Because it asks different questions, this book differs markedly from other "pictorial histories" of American cities. Instead of emphasizing society and customs, this volume deals with the physical conditions of life. In place of the conventional interest in "founding fathers" and leading families, it is more concerned with street scenes and ordinary people. Without neglecting the downtown, it also reaches into the residential areas and neighborhood shopping centers. Moreover, this volume is concerned with suburbs and "satellite" towns as well as the historic city. It encompasses the area from Waukegan on the north, to Elgin on the west, to Gary on the east. In short, it tries to reconstruct metropolitan Chicago and to see it as it appeared to successive generations of residents and visitors.

Included in CHICAGO: GROWTH OF A METROPOLIS are five handsome fold-out panoramas of the city which were made at different times in the city's history and reproduced in facsimile of the contemporary photographic process. Luxuriously bound, slipcased, 528 pages, \$27.50 (after January 1, 1970: \$32.00) LC: 68-54054.

The University of Chicago Press, Chicago 60637

A Discourse of the Commonweal of This Realm of England

Attributed to SIR THOMAS SMITH. Edited by MARY DEWAR, University of Texas at Austin. xxvi, 169 pp. \$6.50

This edition of *A Discourse of the Commonweal of This Realm of England* is the most accurate available; the various manuscript copies extant have been collated to approximate the original text as closely as possible. *A Discourse*, though written in 1549, was first published anonymously in 1581. It was part of the flood of "commonwealth literature" provoked by the severe inflation and changing economic patterns of Tudor England. *A Discourse* stands as "the most advanced statement of economic thought in Tudor England."

The Case of the Commonwealth of England, Stated

By MARCHAMONT NEDHAM. Edited by PHILIP A. KNACHEL, Associate Director of The Folger Shakespeare Library. xlii, 151 pp. \$6.50

In 1650, shortly after the overthrow of Charles I in favor of Independent rule, Marchamont Nedham, one of the best-known journalists in England, published *The Case of the Commonwealth of England, Stated*. Nedham wrote this tract to prove "the equity, utility, and necessity of submission to the present government." He hoped it would persuade the people to abandon their hostility toward the new Commonwealth. Nedham's book is republished here for the first time since the two original editions of 1650.

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